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ESTUARINE WATER QUALITY MODELING STATE-OF-THE-ART. TECHNICAL CONFERENCE (1ST) HELD AT ANNAPOLIS, MARYLAND ON JUNE 24, 1969

TRACOR, Incorporated
Austin, Texas

24 June 1969

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ESTUARINE WATER QUALITY MODELING

STATE-OF-THE-ART

FIRST TECHNICAL CONFERENCE

St. John's College
Annapolis, Maryland

24 June 1969

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1. INTRODUCTION

This document is a transcription of the proceedings of the first technical conference on the State of the Art of Estuarine Water Quality Modeling held June 24, 1969, at St. John's College in Annapolis, Maryland. This conference formed a part of the effort to prepare a report surveying and summarizing the state-of-the-art of water quality modeling as applied to estuaries. Preparation of the report is currently being undertaken by the Federal Water Pollution Control Administration through contract with TRACOR Inc.

The purpose of the Annapolis conference was to review present FWPCA activities in the area of estuarine water quality modeling and to decide upon the organization, scope, and content of the projected report. With regard to the latter, the conclusions of this first conference may be briefly summarized as follows:

1. The outline the report will assume is
    - I. Introduction
    - II. Hydrodynamic Models
    - III. Water Quality Models
        - A. Waterborne Constituents
        - B. Temperature
    - IV. Physical Hydraulic Models
    - V. Solution Techniques
        - A. Analog and Hybrid Techniques
        - B. Digital Techniques
    - VI. Case Studies
    - VII. Discussions and Alternative Viewpoints
    - VIII. Conclusions and Recommendations
2. The State-of-the-Art report should not be a summary of the literature prepared as a handbook

> or guideline to workers in the field, but rather a succinct assessment of the development and the present status of water quality modeling, including the merits and limitations of the various models employed and the merits and limitations of the techniques utilized for their solution.

A concerted effort has been made to prepare as complete and faithful a transcript as possible. Nevertheless, some editing was necessary.

The original suggested report outline and the chapter outlines Dr. Harleman provided are included in the appendices. These were mentioned repeatedly in the conference, and are included here for convenience and completeness.

2. CONFERENCE PARTICIPANTS

Donald J. Baumgartner	FWPCA National Coastal Pollution Research Center
Richard J. Callaway	FWPCA National Coastal Pollution Research Center
Joseph L. Collins	TRACOR
William E. Dobbins	Teetor-Dobbins
William H. Espey	TRACOR
Kenneth D. Feigner	FWPCA Southwest Region
Daniel Fitzgerald	FWPCA Northeast Region
W. H. Fitch	FWPCA Headquarters, Systems Analysis Branch
Thomas P. Gallagher	FWPCA Southeast Water Lab
James A. Harder	University of California at Berkeley
David R. Hopkins	FWPCA South Central Region
Robert C. Horn	FWPCA Middle Atlantic Region
Robert J. Huston	TRACOR
Norbert A. Jaworski	FWPCA Middle Atlantic Region
Malcolm F. Kallus	FWPCA South Central, Galveston Bay Office
Donald J. O'Connor	Manhattan College
George D. Pence	FWPCA Northeast Region
Donald W. Pritchard	Johns Hopkins University
Maurice Rattray	University of Washington
William C. Shilling	FWPCA Robt. S. Kerr Water Research Center
Roger D. Shull	FWPCA Headquarters, R&D

Robert V. Thomann	Manhattan College
John Vlastelicia	FWPCA Northwest Region
George H. Ward	TRACOR
T. A. Wastler	FWPCA Office of Estuarine Studies

3. CONFERENCE AGENDA

TUESDAY, 24 JUNE 1969

Opening Remarks and Introductions

8:30	TRACOR
8:45	Baumgartner
9:00	Wastler

Reports by FWPCA Representatives

9:15	Pacific Northwest Water Lab, Callaway
9:35	Northeast, Pence
9:55	Mid-Atlantic, Jaworski
10:15	Southeast, Gallagher
10:35	BREAK
10:50	South Central, Kallus
11:10	Southwest, Feigner
11:30	Northwest, Vlastelicia
11:50	Summary Comments, TRACOR
12:00	LUNCHEON

Afternoon Discussion: Report Outline and Content

1:00	Statements by consultants
2:00	Open Discussion
5:30	Concluding remarks, TRACOR

Dinner at the Naval Academy Alumni House

4. MORNING SESSION

ESPEY: Why don't we attempt to get started this morning, a little bit late. I would like to introduce myself. My name is Bill Espey, and I am with TRACOR, which has the pleasure of being the contractor which is to basically provide the technical editing support for this little report, as well as participate in some of the technical chapters. With me today are some of the members of our team from Austin; I won't introduce all of our people. This is Bob Huston and George Ward. And also my boss, Joe Collins, the big fellow over there, from our Austin office.

I would like to just briefly go over a few of the physical arrangements, the events of the day. We will have the conference in this room throughout the day. There is a little supper arranged this evening for us at the Naval Alumnus Center, or House, I am not sure what the name is, which is over on the corner, directly across from the school. It is on the little map. I think this will be a rather interesting evening. That is quite an historical spot, and I think the food is excellent. We will also have a luncheon at the University here at about 12:30. There are ample accomodations across in the dormitory for those that have come in this morning that would like to stay this evening. I think quite a few are going to be leaving that stayed last night, for one reason or another.

(Laughter)

There will be coffee at 10:30 and doughnuts in the little reception area, also.

We would in general like to accomplish this morning a review of the FWPCA program. Most of the authors have been provided with what we hope is ample detail to describe the agenda. Each region will present a summary of their activities (the tone will be very informal) to bring you up to date on the activities of the various laboratories in the various regional offices.

This afternoon will be devoted to primarily a discussion of the report itself. The authors, we hope, will be able to come up with a final decision on chapter content and scope of the report, which we hope to deliver in approximately six months to FWPCA. We have sent to you a tentative outline, and I underscore tentative. We feel like it is fairly comprehensive, but still is a suggested outline for your review and approval. We hope to be able to end the day's events with a fairly well-defined outline with which each author can then retreat to his little domain and prepare his chapter, his area of responsibility; in approximately three months, have another meeting in which this document will be reviewed quite extensively, and before that meeting, have transmitted the rough draft to each participant so that he might be prepared for the second meeting.

The participants, consultants if you please, in this program, I would like to just briefly introduce each of those who are here today. I think many of you know them by reputation. Dr. Pritchard here on my right, Dr. Harder, Dr. O'Connor, Dr. Thomann, Dr. Dobbins, Dr. Rattray, right next to George here. I hope I haven't skipped over someone; I think that is all the major consultants that are here today. In addition, there are also some gentlemen that will participate, unfortunately they were not able to get here for some obvious reasons, it was just a short time for them to make plans. I again sincerely appreciate each of the individuals, both federal and non-federal, for their very courteous attitude and help in the preparation of this conference, as well as aiding us in formulating some of the outlines in the agenda. We have had limited time to prepare for this conference, but as I understand, we hope to respond to and get this document to FWPCA and hope that it can be part of their overall report to Congress with regard to the estuarine studies being performed by the Office of Estuarine Studies. This will be elaborated on by Mr. Baumgartner and Mr. Wastler in their remarks. The other participants, I would like to mention to you, that were not able to get here today, and George, if I leave anyone out, don't hesitate to call my attention to it. Jerry Orlob will participate, I believe, but he just could not make it today. Dr. Paulik, biologist at Washington, was not able to make it today. Dr. Keulegan will review the document, but will not be able to get to the meetings. Dr. Ippen, we understand, will also participate. Dr. Harleman, who is in England on sabbatical, will play a major role and participate in two chapters, and George has had correspondence as well as talked with him over the phone, and we have some thoughts on what he feels is appropriate for this report. George will transmit these to the conference and present you with a little outline that he presented to us. Dr. Masch of the University of Texas is also not here. He is a reviewer as well as Dr. Paulik, Ippen, and Keulegan. The tentative outline of the chapter and the corresponding authors, of whom fortunately many are here: Dr. Pritchard will write an introductory-type chapter, the Analog-Hybrid section of the report will be authored by Dr. Harder, the Digital section is in general at this time broken into two categories, a Hydraulic section with Dr. Harleman taking the key role there, and a section on Water Quality Modeling co-authored by Dr. O'Connor and Dr. Thomann, with possibly a subsection on Temperature Modeling. The Physical Hydraulic Section will be prepared by Dr. Harleman. A Case Study section will be prepared by my company and our technical people. In addition, there will be a section which will bring forth the various alternative views that we expect to be generated throughout the discussions both today and in the second conference. I am sure there is some disagreement, possibly some different views that will be brought out in this chapter. Then a little chapter on research that needs to be done.

So again, we hope to review FWPCA's program this morning and this afternoon attempt to get down to a very detailed discussion of the chapter content, outline, scope of the report so that we can leave here with a fairly good idea of what each individual participant is supposed to accomplish within this three month interval before the second meeting.

I think I have gone over my little list. I would like to underscore that the meeting is informal and ask if some of you gentlemen would like to take off your jackets and loosen your ties, and relax.

Let me just very briefly read the two major purposes of the document of the State of the Art: to provide technical guidance to the National Coastal Pollution Research program of FWPCA and to be included as a technical supplement in the National Estuarine Studies report prepared by the Office of Estuarine Studies. As I understand it, there may be some problem in meeting the deadline for this report and Don will elaborate on that, and some of the broader objectives of the study. I will just pass that on to him.

* * *

BAUMGARTNER: Thanks, Bill. I would like to introduce those people who are here from FWPCA in case everybody doesn't know who they are. George Pence and Dan Fitzgerald over here on the left are from the Northeast Regional office. We brought our big man along too, Mal Kallus back there,

(Laughter)

next to Chris Shilling from the South Central region. Over here Al Wastler from the Office of Estuarine and Oceanographic Programs in Washington. Dick Callaway from the Coastal Pollution Research staff in Corvallis; John Vlastelicia from the Northwest Region, Roger Shull from headquarters, Norb Jaworski from the Mid-Atlantic Region, Tom Gallagher from Southeast, Ken Feigner representing the West Coast (Southwest) Region. And let's see who we have there: Dave Hopkins is from the South Central Region too; another gentleman from headquarters, Bill Fitch, and Bob Horn on the back row from the Mid-Atlantic Region. Now is that everybody from FWPCA? O.k.

Just a little introduction here about what the National Coastal Pollution Research Program is. This is one of the operating branches of the Pacific Northwest Water Laboratory, a Division of the Northwest Region of FWPCA. Like other branches at this lab and similar FWPCA laboratories around the country, the research program is national in scope. While our mission is to develop the methodology for preventing pollution of coastal waters and recovering the water uses from regions that are already polluted, we aren't the only ones who contribute to this end. The Marine Water Quality Laboratory in Rhode Island, for instance, conducts research on the effects of pollutants on marine organisms, and conducts research on determining what are the water quality requirements for marine water uses. Information on a number of other of the 65 research activities conducted by the R&D program, such as oil pollution, analytical methods, biological indicators, are also contributed by other branches at the laboratories and at headquarters.

Approximately 80% of the administration's R&D budget is specifically allocated to grants and contract research. The major task of our Coastal Pollution Research program is to determine the fate of pollutants discharged to coastal waters. By this I mean the transport, dispersion, physical-chemical-biological interactions the wastes undergo with the environment, and the impact on marine water quality. This is to be distinguished from the effect on marine water uses. At this point, we interface, if you will, with the Marine Water Quality Laboratory in Narragansett, and there are many topics which we have to distribute between the two laboratories.

We need this technology to analyze any given pollution problem in the coastal waters. To determine the relative contribution from a number of sources which might be in the area, to evaluate alternative remedies which may be suggested for controlling the pollution, and to simulate or predict what might happen in the region from additional changes in the shape of the estuary or from additional discharges into the estuary.

About a year or a little more than a year ago, we decided it was appropriate for us to review our current technology in this area and to suggest what future research we might want to support on this topic. Primarily, we wish to analyze the advantages and limitations of hydraulic physical models and mathematical models, including solutions by analog and digital-hybrid computers. This, we felt, would be most beneficial if it were prepared by a group of consultants and specialists who were very familiar with the technology. At the same time, we wanted to make sure that this assessment was directed to the problems that FWPCA was working on. This work is primarily done by members of our regional office staff in technical operations, and we felt that they would be beneficial to this discussion since they could tell us what their capabilities were, what kind of modeling they have been employing, what problems they were directing them to, and what deficiencies they saw in this current technology which they would like to see overcome by future research. In addition, two problems were suggested by the Office of Estuarine and Oceanographic Programs, the effects of streamflow modification on estuarine water uses and the effects of physical modification on the same, and with some modification in scope, these were included into a plan for preparing this state-of-the-art report. After this report is prepared, we wanted to have an opportunity for it to be reviewed by the people who are the ultimate users, to present questions, to ask for clarification, and not only for them, but for the other technical consultants to provide, if any, opposing viewpoints.

As Bill mentioned, this report will be of use to us in guiding our research program, and although this was originally planned to be issued as, or included as, a supplement to the Estuarine Studies Report, this was when we were thinking of having this meeting as early as last January. Due to contracting difficulties, we were not able to do that, and I am not going the answer the question that Bill posed as to whether or not this is still possible. I will let Al Wastler say something about that if he so desires. With that brief introduction, I'll conclude my remarks unless someone has some questions about the program. Bill.

* * *

ESPEY: The next speaker on our program is Al Wastler of the Office of Estuarine Studies, and I'll just turn it over to you, Al.

WASTLER: Coming up in November we will be presenting to the Congress a report on the National Estuarine Pollution Study, which was directed about two and a half years ago. One of the major recommendations, the major recommendation in the report, is to be a recommendation for a national program for the study, use, and investigation of estuaries, not just a federal program, but a national one, which will include the responsibilities of the state and federal government and the public and private sectors of the country. One very important aspect of this report will be to lay out a program of research and study, investigation, of estuaries. One part of this program, rather obviously at this stage, is going to be a nationwide, or coastwide that is, program of basic data collection and use, which will undoubtedly have to be done probably on a cooperative state-federal basis. Since one of the major means by which estuaries are studied and will be studied is by modeling them, either mathematically or analog or hydraulically, we would like to develop some kind of information on what the present state-of-the-art is with regard to modeling so that this can serve as a guide to establish a framework for basic data collection and utilization.

One of the problems that we have found that has existed for a number of years is that there are many independent investigators proceeding and doing the best they can with what information they can collect and by whatever means they can collect it. This has led to a great number of partially-done studies, programs and that type of thing. If possible, we would at least like to lay the groundwork and make a recommendation to put our data collection efforts on some kind of cohesive, integrated basis. At the same time, we would like to recommend to the Congress additional support in modeling work, where the support is needed and where it can bring the most useful information. So that's what we're after basically, some kind of overall framework which we can use to make recommendations for a cohesive program of modeling utilization in the estuarine zone.

As I said, our report is due in November, November 3 to be exact, almost October. We are going to attempt to get into the report all the information that is available up until the end of September if possible. This does not mean that we are not going to write the report, in fact, we are writing it right now, but we will try to hold it as open as we can for additional information. Hopefully, we will be able to see some of the draft material before the final report comes out so that we will know which way things are going and can program it in. Whether we will be able to hold our report open to actually refer to this report as a final product, we can't say. We recognize that there may be some slippage in the time the final report comes out, and we hope there won't be any slippage in ours, or there might be a few angry congressmen on our necks. But, anyway, we will try to get into the report as much information as we can of what you are developing here. I would just like to stress just once more that our main object is to get an appreciation of the present state-of-the-art so that we can make recommendations which will result in a cohesive program of data collection for utilization where it can do the most good.

* * *

ESPEY: Thank you, sir. We will get into the good part now, from the various regional labs. We would like to start off with Dick Callaway of the Pacific Northwest Water Lab, who will talk on his activities. I think he has some slides for us.

CALLAWAY: I just want to talk a little bit about an in-house effort that we are doing on the Columbia River model, the Columbia River estuary so-called, from the ocean to the bottom of the dam. This is a schematization of the estuary from mile 28 to mile 146, which is the bottom of the dam. The reason we started at mile 28 is because salt water penetration ends at mile 25, and that's where everybody stops in these things if they've got any good sense.

PRITCHARD: Then you are using estuarine models in a non-estuarine environment.

CALLAWAY: No, there's still current reversal up to about mile 90.

PRITCHARD: That makes it a tidal river.

CALLAWAY: All right, if you say so. Anyway, this estuarine model goes from about 120 miles upriver. The low flow in the Columbia is about 150,000 cfs, a situation somewhat different than in the East and Gulf Coast estuaries. Current reversal takes place, like I said, up to about mile 50 or 75, depending on the volume of flow. The schematization represents 260 junctions and 276 channel elements that are used to simulate the area. Let me show some slides of potential attempts at verification. This is taken from Dr. Orlob's, of Water Resources Engineer's program, created from that, and using a sine-wave input for the tide at the lower end and observed a bulge along the estuary. This is high tide and current speed at different river miles, from mile 28 to mile 42 and about 54.

PRITCHARD: Are these observed?

CALLAWAY: No, these are computed.

PRITCHARD: O.k.

CALLAWAY: Actually, convergence hasn't been reached because we didn't let it run long enough, but you can see there is a phase lag going upstream for that part.

PRITCHARD: Both the tidal height and current surge computed, so I understand.

CALLAWAY: Right. These are computed values. This is further upstream, mile 89, 90, 114. This, to repeat again, hasn't really converged because we didn't let it run long enough. And here's one more. This is even further upstream, mile 114, 130, and 144.

It is kind of hard to verify something like this because there just isn't any data around to do it, but this is one way you can do it. The solid line is the phase lag in tide height from the Coast & Geodetic Survey tide tables, here, based on 0 lag at Jim Crow Point, and the x's are the computed phase lag going upstream.

HARDER: Was there some problem of the amount of computer time that you had available?

CALLAWAY: No, I just didn't get time to do it before we came here. And I cut it off for one thing. This is fairly good verification, but there really isn't enough.

We're also trying to model the lower part, Jim Crow Point here. Jim Crow Point is where the other model starts, it goes upstream. This is fresh water all the time, salt water penetration about here, mile 25. I don't have a scale on here, but this is about 5-10 miles across. This is the Pacific Ocean, and this is the grid, looks a little strange, but these are fairly large tide flats here. We haven't run this part yet.

PRITCHARD: This is some sort of a branch system, that we see, which is used in this model?

CALLAWAY: Some sort of which?

PRITCHARD: Branch system?

CALLAWAY: Right. This is taken from Orlob's channel model.

HARDER: What do you see as the principal problem that you are trying to solve here in this study? Tidal heights and currents in the estuary?

CALLAWAY: Right. The reason we are running this stuff is to get a temperature model of the whole Columbia River, using this hydraulic part to get the velocities as input to the dispersion equation.

PRITCHARD: Except for branching, is this essentially a one-dimensional model?

CALLAWAY: Yes.

PRITCHARD: It doesn't include the vertical?

CALLAWAY: No, its velocities are uniform all the way across, like that, straight down.

O'CONNOR: What criteria did you use for that particular branch system that we're looking at?

CALLAWAY: Stability criterion for this, and the reason it looks a little weird here is because of the tide flats. So we are assuming that the flow takes place just along from here to here, or say, from here to here, from here to here. It is quasi-two-dimensional. A two-dimensional model would give you speed and direction anywhere like this. This one is kind of constrained.

O'CONNOR: Are these hydraulic models necessary to answer some of the quality problems that we have?

CALLAWAY: What we're trying to do is solve this equation:

$$\frac{\partial T}{\partial t} = u \frac{\partial T}{\partial x} + \frac{\partial}{\partial x} (K_x \frac{\partial T}{\partial x}) + \Sigma Q$$

You will notice that this term is input to the temperature and this is the extraction. We've gone to a lot of trouble to just get the intertidal velocities.

O'CONNOR: Yes, that's what I mean. We've gone to a lot of trouble just to get one number. Is one number that critical to the problem we have here?

CALLAWAY: Yes, I think so.

THOMANN: The time scale here, Dick, is on the order of hours, is that right?

CALLAWAY: No, it is on the order of minutes. We're not averaging over a whole day.

HARDER: So that's the time scale of your calculations, what about the time scale of the temperature fluctuations?

CALLAWAY: That's something else to worry about. We hope we can get away with one hour inputs.

HARDER: In terms of the problem of the temperature, is that a problem that's a daily problem, or a weekly problem, or an hourly problem?

CALLAWAY: Well, that's hourly too. Because the radiation term varies quite a bit from hour to hour, day to day, and week to week. If you use daily averages for radiation, you can get away with a model like that, but I don't think it would be useful.

The initial purpose of each of these junctions is that you can put in a temperature heat input from [illegible]uclear reactor or something that is discharging. But actually the area involved here may be on the order of one square mile, so it can't be a point source here, assuming it is completely mixed instantaneously over a couple of square miles wouldn't really make much sense. So when you have a situation like that, you really have to go to a much smaller grid size and time step, which we haven't done.

THOMANN: What sort of machine time are you running, for, say, a tidal cycle?

CALLAWAY: This is with a 3300, and for a 13 hour simulation, I think something like seven minutes, which is a long time.

QUESTION: What computer was that?

CALLAWAY: CDC 3300, it's fairly small. I think Ken Feigner has been running essentially the same program with maybe not that many junctions, but I think he did one or two minutes.

FEIGNER: Yes, this is on a CDC 6600.

THOMANN: How many minutes?

FEIGNER: Approximately three. The system's probably not quite that large.

THOMANN: For the same length of time, 13 hours?

FEIGNER: It would be for probably a longer time interval, probably about 25 hours. Actually, you are more on the order of 35 to 50 hours, in other words, two to three tidal cycles, the last of which would be approximately a steady-state.

O'CONNOR: Are there any other quality problems you have up in this area, Dick?

CALLAWAY: Well, I think John Vlastelicia will tell you about those.

Some of the questions I'd like to raise are the following. Some of the recent work on numerical mixing has shown this can be a pretty serious problem if not taken into recognition, such as the numerical mixing that you get from the round-off error and so forth that's wrapped up in the diffusion term. Then again I just wonder just how useful a strictly deterministic model like this is. It's tough enough to verify, and once you've verified it for a certain period of time, what good is it outside that period of time? This is largely a matter of judgement, I suppose.

The third question is what's the best way to verify the thing? Are James River-type surveys required for large varieties of river flows, or just what is the optimum method of verification? And fourthly, with a numerical model of a stratified estuary and a pollution model of a stratified estuary, how good can we get with these things at this time? I know you can get numbers from them, but what do they really mean? I think that's all I have, then.

GALLAGHER: What are the consequences of power liberations on temperature in an estuary in terms of water quality?

CALLAWAY: Consequences?

GALLAGHER: Consequences of power liberations on water quality.

THOMANN: You mean from an ecological point of view?

GALLAGHER: How do variations in temperature affect the water quality?

CALLAWAY: Well, I think it depends on what part of the estuary you mean.

GALLAGHER: O.k., in the Columbia.

CALLAWAY: I don't know if I understand the question.

JAWORSKI: Well, the point here is, so you have got a two- or three-degree machine fluctuation, what the hell does it really mean here in terms of let's say the DO parameter, or the spawning of a certain kind of salmon. What does it really mean 20 feet down the channel? Can you get by with a daily simulation, so you're off one degree or two degrees C...?

CALLAWAY: I think this depends on what your objective is.

VLASTELICIA: This is the subject of a two-year study in the Northwest Region on thermal effects right now. There are 23 biological studies being conducted to answer that question, as to what does a 2° change in temperature mean right now. And the idea is to implement water quality standards on the Columbia River, and there are some differences between the State of Oregon and the State of Washington as to what these standards might mean. But it has already been determined that if a 2° change in the temperature occurs over a short enough period, it will kill a fish. And this stems largely from, I think, nitrogen supersaturation in the Columbia as a particular problem. For this reason, I think that Dick's had to go to a model that will vary from hour to hour to test different patterns of discharge up and down the Columbia River, and how these get together in a tidal cycle and heap up as it floods through back and forth past them.

CALLAWAY: The usual pattern has been to take 10 day averages, whatever that means.

JAWORSKI: Can you refine that thing down so that it's less than ½°C, would this be what you're after?

GALLAGHER: How sensitive is it?

BAUMGARTNER: When we started on this, I think it was anticipated that the maximum time period that we would want to average over would be six hours, and we may want to go as short as three hours, two hours, and one hour, and it depended a little bit on what kind of change in temperature they got with different types of input and reservoir operation.

So I think the biologists were tending to push for shorter time periods so that they could get more of a control on preservation of fish and the eggs, but at the same time, we didn't know what kind of response, or what kind of sensitivity the system would have for various inputs. So that's why we just couldn't assume when we started that we'd be satisfied with the daily average.

We were somewhat hampered by the meteorological inputs to the heat balance equation because there we have probably six hour averages.

THOMANN: Well, I think that, Dick, you had some sort of reservations, and I think I have them too, about using these kinds of deterministic models on those kinds of time scales. Data on those time scales are responding to environmental influences that tend to be quite stochastic. And that is where the verification problem really raises its head.

CALLAWAY: As I mentioned, using a model that represents 120 miles of river and each junction represents 2 to 4 square miles, and using this with putting in a heat source from an outfall doesn't make a great deal of sense. You have to go to a much smaller grid size.

THOMANN: Well, it makes sense, if you have quite a bit of mixing.

CALLAWAY: Well there is 150,000 cfs. You can lose a heck of a lot of pollution in something like that. It's just like tying a string to the Empire State Building and yanking on it to move it. It won't. So, the main use for this is mainly to predict, initially, what the effect of different flows are going to be on the tides, on the currents and so forth, and the volumes of water involved. But if you really want to predict within a mile of the outfall, this kind of model is not too good, the grid size is too coarse.

HARDER: You mean that essentially your thermal pollution from a power plant would not extend more than one or two of your mesh points from the source?

CALLAWAY: Right, We were operating under the "constraint" that we had to model the Columbia from the Canadian border to the ocean.

HARDER: So from the standpoint of the power plants, just a mile or two from the plant would be adequate.

CALLAWAY: Well, approximately, yes, but the other point may be that all the heat problems are coming from the Bonneville dam, in which case we'd be in pretty good shape to predict that, rather than putting in little point sources along the estuary.

HARDER: You have kind of a two-stage problem, a local problem and an overall?

CALLAWAY: Yes.

PRITCHARD: This is what I wanted to ask. It seems to me that if you're concerned, and I think I've seen at least some of this work reported in other conferences also, in which the subject is to develop a model in which the temperature regime along the length of the Columbia under various release flows from the reservoirs, etc., are included. Here you're concerned with what you might say is the natural variations in the input, and so you are concerned with looking at the radiation terms and all the heat balance terms from the standpoint of predicting the temperature. When you come to an input though, a power plant input, this kind of model even when you scale it, even when you go to a small grid, is not going to be very effective, because the mixing characteristics of the effluent into the waterway is so highly determined by the hydraulic characteristics of the discharge itself, that is, as far as what are the characteristics of the discharge structure, heated release, the ratio of the transverse jet velocity versus the ambient velocity along the stream, etc. Here what you're doing is putting an added heat input, if you are concerned with what this does to the natural temperature regime, if it weren't there, then you're taking a difference between two heat budgets, the heat budget under natural conditions and the heat budget with this added term, and when you take that difference a lot of the input terms, the radiation input terms, drop out, and you're left with essentially an equation involving the temperature difference. And you're looking for the excess evaporation, the excess conduction, and the excess back radiation which are the three primary terms.

CALLAWAY: Yes, I think that's the way the Japanese handle it.

PRITCHARD: I can't speak for the Japanese, but it's the way it's been handled in actual engineering evaluation of large thermal plants. What I'm really getting at is that when you get to an input of a large thermal source, you could have a model that predicted the general distribution along the system, but then there gets to be quite a difference. I think it would certainly be much simpler to have quite a different model to deal with the local input and its additional effect on this system because, as I say, a number of these terms drop out, and the cooling, the exchange coefficients for the atmosphere involve much simpler considerations, where you are looking at the exchange of the excess only. And this can be reasonably linearized. The uncertainties of linearization are certainly small compared to the uncertainties in the mixing, particularly when you are dealing with an estuary which is stratified. When you try a one-dimensional model, I think you run into problems.

Could I ask one other question? This model, I think I've read something about the background of this, but this involves the use of a combination of the dynamic equation reduced to one dimension and the continuity equation, and to which there is a frictional term, and I've forgotten, how is the frictional term expressed?

CALLAWAY: As a Manning coefficient, n.

PRITCHARD: As a Manning coefficient? Well, there's a little difference in how the velocity appears. Is it a squared term in the velocity?

CALLAWAY: Right.

PRITCHARD: I would say that is a Chezy coefficient. In other words, essentially, the frictional term is expressed as a $|u|u$.

CALLAWAY: Yeah.

$$\frac{\partial u}{\partial t} + u\frac{\partial u}{\partial x} + g\frac{\partial h}{\partial x} + k|u| = 0$$

$$\frac{\partial h}{\partial t} = -\frac{1}{b}\frac{\partial q}{\partial x}$$

This is an explicit solution, and that's why you have problems with stability. And attempts to slow the wave down by increasing the Manning coefficient gets you into trouble too.

PRITCHARD: You use Manning coefficient to convert to this k, but the k certainly is not the Manning coefficient.

CALLAWAY: No.

O'CONNOR: I think this problem of scale, which was brought up, is a very significant one. I'm inferring from what you said, and I'm agreeing with you, that the problem of modeling might be a two- or three-dimensional one on a much smaller scale than the one-dimensional large scale that you suggested here. I think that is what Don was getting at.

THOMANN: In fact, this problem will probably pervade our discussion all day, the question of scale in both time and space.

CALLAWAY: This model is one-dimensional, which assumes the velocity is the same from top to bottom, two miles across and all the rest of the other things that we'd like to forget about.

BAUMGARTNER: I don't want to give the impression that we didn't recognize this. We recognized this from the beginning. However, there was the requirement to determine what the effects of additional heat input and reservoir operations would be on the complete Columbia River system, and they decided in our regional office that this was of sufficient importance that we should attempt to do this first. From the beginning, Dick always recognized that there was a local problem which was probably much more critical.

CALLAWAY: I said to myself every day you can't do this.

(Laughter)

BAUMGARTNER: When he finally convinced himself he could, he said it's not important.

(Laughter)

CALLAWAY: Doesn't everybody get to that stage?

PRITCHARD: I certainly recognize the value of large scale modeling to look at some problems which are large scale, for instance, the total effects on the temperature distribution along the Columbia River because of variations in operation, the impounding problems, etc., and these kinds of models are also useful for instance, if one is concerned very grossly with the mean salinity across the section under various river flow regimes. Even in a two-dimensional or a three-dimensional estuary, it is surprising how well a one-dimensional model will give you things like the mean salinity distribution, as you are familiar with the segmented model. But, it really doesn't tell you what happens to a local source.

* * *

ESPEY: From Northeast, George Pence. George.

PENCE: I will try to follow the outline in DickCallaway's memo in hopes that it will make things a bit quicker. In general, we have three different programs that I want to talk about. The first is a program that we call PRINE. There is a version of it which is available in the Middle Atlantic region. It is a steady-state segmented one-dimensional estuary model, written primarily for dissolved xoygen and BOD. It uses a matrix inversion technique for solution of, in our version, up to 30 sections. These models are based on the assumption of cross-sectional homogeneity, which we seem to follow fairly well in the estuaries we have modeled so far. This PRINE is our workhorse program in that it forms a basis for economic modeling that we do. Adding to PRINE the cost curves and loads from various effluents, these economic models then will allocate loads according to any of three different economic constraints, either a least-cost solution, where each effluent is treated on its own basis, and whoever removes the most for the least amount of money gets tapped for it. There is a uniform treatment model, which is a much easier concept; it's really almost a trivial solution in that you step through various levels of treatment until you arrive at whatever goal you had in mind. And then there is a zone treatment model which is sort of a combination of the two in that you constrain removal percentages to be the same within specified zones. These zones can be geographical, and/or industrial-municipal, or something like this. This model was developed in-house with contracted programming on it. The assumption again is a cross-sectional homogeneity and completely mixed within sections. These sections in practical use were ten to twenty thousand feet long. The time average used on this again is steady-state. When we talk about steady state, we're talking about a period of, in practical use again, three months; one, two, or something like that, but at any rate on a monthly time scale. This model has undergone extensive verification for DO-BOD. It's been in use for four or five years on a regular basis. The documentation that exists on it is partial from us, that is, the Northeast Region. When the Delaware Estuary Study report comes out, it will be fairly complete. I would recommend, however, obtaining documentation from the Middle Atlantic Region. They have rewritten, modified this program quite extensively. It is much easier to use, and they have complete and rather nicely done documentation. The limitations of the model are primarily that it is a steady-state model, and as currently written, is limited to thirty sections. It was written for the Delaware, that is all we needed, and that's all we got. As far as other regions using it, they can and are using it presently. The Middle Atlantic, I believe, is using it on the Potomac and the James. And further application on this particular program will be limited to applications, probably not much more development on it as such.

The second model is similar in nature; we call it DECS-III, a time-varying model, however, instead of using a matrix inversion solution, it uses a Runge-Kutta integration on up to 150 sections. Again, this model was developed in-house with outside programming. It has been the subject of considerable revision over the past three or four years. It has the same assumptions of cross-sectional homogeneity and completely mixed sections, which was found in the steady-state program. It is again primarily written for BOD-DO systems; however, it works very well for salinity and it has been used for dye, coliforms, alkalinity, acidity, nutrients, etc. to a very limited extent. I also believe it can be used for heat, although there will have to be some pretty gross assumptions made. The time scale used with this model is a dialy average. We felt during the development of it that there was no need to go within a tidal cycle. We are interested in modeling trends over the year, more or less, instead of talking about what happens for the two hours immediately after sunup, or something like that. We are more interested in what happens with a certain treatment program over some hypothetical flow year, and if the stochastic variations about that are desired, then we can add them, superimpose them from time series analysis work, etc. The verification that has been done on this has been primarily limited to DO and salinity. It was used quite extensively during our 1965 drought period for modeling salinity, and this is our basis for saying it works pretty well. We have complete documentation on it, and this program, again is and can be used in other regions, again the Middle Atlantic Region is using it and a modification of it which was written specifically for salinity, both again I believe on the James and the Potomac. There will be further development of this program. I don't believe that the Northeast, at present, will be directly involved. I say directly involved, we have given this program to the Delaware River Basin Commission, and they are going to be doing further extensive verification with it and perhaps, as we go along, some reprogramming work. Currently when it is run now, you have to run the first stage BOD, and then a separate run for the second state, and superimpose them. We would like to develop a program whereby we run them all at the same time.

The last model program that we are using currently is the program we call the New York Harbor model, which is a steady-state estuary model, quasi-two-dimensional in this branch. It uses analytical solutions within sections, the sections being based on continuity of loading and physical parameters, cross-sectional area and so forth. It is a BOD-DO model, again cross-sectional homogeneity is assumed. The amount of work that we have done, verification that we have done on it, is extremely little. We have had this program for several years, but it just never has been used. We are currently planning in the very near future to begin using this program on the tidal portion of the Raritan River and on the New York Harbor complex. Documentation on this is rather complete on theory, but as far as the program goes, the documentation is non-existent. The program is limited at, I believe, 47 equations, and (I'm not quite sure how to say this) we find that there is a good bit of trade-off, if you will, between this and the PRINE model that I described before, in that there are situations which, due to their physical characteristics or load configurations, one of these models may work out to be better than the other, easier to use perhaps. We'll be using it on the New York Harbor and the Raritan, but it may conceivably turn out that we'll look at PRINE for the Raritan River also. This, of course, has the added advantage that it is quasi-two-dimensional which PRINE is not. I can see no reason why this model cannot be used in other regions. I believe, Don, it has been used elsewhere, has it not? We will be doing considerable future application, possibly development, of this model.

Also within the region we've done a considerable amount of this limited in-house analytical solutions in small estuaries and so forth. We also are planning in the future on using the San Francisco Bay model, hopefully in the Boston Harbor area. One sidelight. We have, using a similar set of equations to the time-varying model, solved this problem on an analog computer on three different occasions. The first occasion was 1965, trying to model DO-BOD. We were not particularly successful, the conclusion being reached that we had too big a problem for the analog computers available at the time. We ran 360 amplifiers which at the time was three of large analog computers, and we found that these things, much like my stereo at home, were always blowing tubes, and something of that sort. Subsequent to that we ran a smaller problem modeling salinity and had very good results with it. We also ran an even smaller problem yet on the verification of a dye study and also had good results. I could not say that our experiences on the analog computer have been particularly applicable in that they occurred quite some time ago, and I understand that analog equipment is much better now than when we were running.

WASTLER: I remember what you did on that was take the digital model and try to stuff it in the analog.

PENCE: No, I don't think that's quite an accurate way to say it. The digital model had not been developed at the time we ran the analog program. But the digital model is simply a method of solving 60 simultaneous linear equations. All we did on the analog was to put those same equations, granted, on the analog, using the analog integrator itself.

THOMANN: The distinction is really arbitrary, you solve the equation whether you use either an analog or a digital.

PRITCHARD: I have a comment about the terminology when we talk about a digital model or an analog model. The model is the math. The model is the symbolism which you've developed, the functional relationships you are going to consider to be important, in parametric form, and then the computers are simply means of solving this. In one case you simulate the functions by RC and other networks, and the other case you take the digital difference forms.

HARDER: That's the case where you're using it as an analog computer. There's a slight distinction you might make between analog computers and analog simulators. The essential difference is between an explicit solution, which you're discussing, and the implicit solution, which is gained from the analog simulator.

PRITCHARD: But you can write an implicit solution on the digital computer, too. It's just a matter of how you formulate the problem.

THOMANN: That's right.

HARDER: What I mean by explicit solution is that you write down all of the equations and solve these equations either by digital or analog computer. What I mean by implicit solution is that you model the system with electric circuit elements which do not solve equations.

PRITCHARD: But that's just hiding behind the bush, because you've got to know how to translate the hydraulics to the electrical analog.

HARDER: That's the most important...

PRITCHARD: And to do this, you've got to assume some parametric form.

HARDER: Well, the most important problem is to define the problem. Not to solve the equations.

O'CONNOR: I think what we are talking about here are definitions of terms, and I would like to subscribe to and second what Don Pritchard mentioned. We're talking about one model and various methods of solution. So far we've only talked about one general model. What we're writing for temperature is precisely the same thing we're writing for BOD and DO, and we haven't seen many different forms of it. That's the one model; we have different forms of solution, so I suggest that, at least I for one, would like to keep that terminology. "Digital models" and "analog models", I think, are distortion in terms.

RATTRAY: It would be much more reasonable to talk about one-dimensional models, or linear models.

O'CONNOR: I think we can understand each other a little better, at least I would suggest that.

HARDER: An analog simulator in your terminology would be much closer to a hydraulic model, a physical hydraulic model.

PRITCHARD: Maybe we need to have somebody..., I don't understand that. Because, for instance, in the analog simulator, you've got to put in a network which essentially assumes the frictional term is somehow related to the velocity.

HARDER: Sure.

PRITCHARD: And you can't change that relation, I mean, unless you physically go in and change it. The analog doesn't make that decision. In other words, you've got to say it is proportional to the square of velocity or something like that in order to build up the analog circuit that simulates whatever you're trying to simulate. In the hydraulic model you don't make that decision.

HARDER: Well, you have other difficulties insofar as you may have laminar flow in parts of the hydraulic model.

PRITCHARD: Not necessarily.

HARDER: Not if you make it big enough, you mean.

RATTRAY: Full scale.
(Laughter)

HARDER: I don't know if this morning is the time to get into the technology of the various techniques, but there are advantages to hydraulic models over computer calculations and equations. And the analog simulator has much the same kind of advantages.

PENCE: Let me tie this thing off by saying that I agree with Dr. O'Connor, we have one water quality model; we have three methods of solution, as a matter of fact, four methods, three of which are digital and one of which is analog.

CALLAWAY: How much money was involved in the analog work? One contract?

PENCE: The first contract was for DO-BOD solution, and it was in the neighborhood of $12,000.

CALLAWAY: When was that?

PENCE: 1964. The second contract for salinity was more in the neighborhood of $3,000 or $4,000.

HARDER: I was interested in this verification. In what way? Did you verify it with some measurements from the prototype?

PENCE: Right, we have monitors, four monitor stations monitoring continuously DO among other parameters. The time-varying model was verified against them and against boat run data for a concurrent period. Of course, we ran into a small amount of discussion on a piece of paper about whether or not we got verification. These monitors were, of course, beside the channel and were used only to show the shape and the relative magnitude of the DO variation over the year. We verified in terms of an eyefit against this, taking in mind the boat run data and so forth. The steady-state model was verified against three month averages of boat run data.

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THOMANN: Could you just very briefly summarize the overall problems that you see you have in the Northeast area? By that I mean what variables are of specific interest to you, and roughly if you can, some comment on the scale of the problems.

PENCE: Well, the variables of specific interest, I would say the most important one, of course, is DO. The big modeling problem with DO would be in the second stage. We have, I think, more than adequate capability to model the first stage. The second stage is another problem. We can model it as it is currently happening. There is to date, as near as I can tell, very limited capability to say what will happen to the second stage BOD-DO relationship when you remove part or all of the first stage; in other words, when you effect an improvement, you improve the DO, what will happen to the reaction of the nitrogen compounds.

HARDER: Does your computer program, or equations, include aeration, and what kinds of sources and sinks of DO? What kinds of details do your equations have?

PENCE: The equations have reaeration coefficients in them. They are linear so you can superimpose loads, and we do include first stage loads, and, as I said, we make separate runs currently for second stage. We can get verification, but this is making assumptions of either a shadow type situation where your second stage load in actuality is discharged here, and in the program you say that in effect it is discharged here because of the lag relationship, or you have changed the decay coefficient such that it is zero or nearly zero where you have no nitrogenous uptake and let them come in later on. So, as I say, we can do this, and we can verify, but who is to say what is going to happen in the future when you remove some of these loads. You no longer have the constraints on the nitrogen system which don't allow it to uptake, and, to the best of my knowledge, we do not now have the modeling capability to say what will happen in the future with this situation.

In addition, in the Northeast we have limited problems with salinity. Many of the water supply intakes in some of the larger estuaries are in not serious danger, but sometimes get into jeopardy from salinity intrusion. In 1965 the Philadelphia intake was, I think the best way to say it, was under panic. I am not sure if it even was under any danger, but there were certainly people worried about it.

Further areas of concern where we have not as yet done any modeling, and hope to see some in the future, would be in the two dimensional or three dimensional bay or harbor situations.

Our problems are more of a long term nature and I think this comes back to what was being discussed before. We are not particularly interested, at least at the present time, in seeing short-range hour-to-hour, or even for that matter, day-to-day simulation of these bay situations. I would be much more interested in seeing a good solid steady-state solution. The modeling involved, the mathematics involved, are not particularly difficult. I think our problems there are more in determining parameters, flow profiles and patterns, diffusion profiles and patterns. I think our emphasis in the near future will be in trying to determine some sort of first cut in getting a good steady-state two-dimensional model.

BAUMGARTNER: Would you say that coliforms, maybe, rather than DO, are more important in Boston Harbor?

PENCE: I am not really familiar with the Boston harbor problems, as such. Dan Fitzgerald is here. Maybe he can answer that.

FITZGERALD: Yes, it seems that so far as standards are concerned, coliforms are of more importance than dissolved oxygen criteria.

BAUMGARTNER: Here the time and space scale might be a little shorter consideration than with DO.

FITZGERALD: Here, still, you would like to have a steady-state solution, too, and you could superimpose...

BAUMGARTNER: You don't think a coliform volume within a day's time is important in a bathing area?

FITZGERALD: Oh, yes. But if you have a steady-state solution, you can impose some kind of stochastic variation to represent what is going to go on possibly in a day's time. You might have a high wind velocity situation, forcing the high coliform level over a large bathing area. And you have to put this random input into it one way or the other, either into this time-varying model as some sort of spike, or the other way.

O'CONNOR: I subscribe to that one hundred per cent. Get a steady-state background and then superimpose something, possibly a delta function.

PENCE: In the Northeast, our philosophy is that we really don't care about modeling present conditions. Our aim is to use these models to reach some sort of decisions on what sort of treatment and so forth is necessary. A strictly deterministic model on a short range situation allows you to say what has occurred, with the data which you know about, and how that was arrived at, but it doesn't mean much when you talk about forecasting what will be the situation in ten years when the wind conditions and the flow conditions, things of this sort, are not going to be the same. You don't know what they're going to be, except that you know they're probably going to vary in some stochastic manner, which hopefully you can forecast from the data.

ESPEY: George, in your verification, has there ever been experienced a situation in which you have made a forecast, and actually implemented the changes, reduced the waste loading, and your forecast proved to be realistic?

PENCE: With salinity.

ESPEY: With salinity?

PENCE: Not with DO or BOD, but with salinity we have. In the 1965 period, I believe that model made some 20-odd forecasts.

CALLAWAY: Is that the one that you ran the hydraulic model alongside?

PENCE: I believe the Corps of Engineers ran the hydraulic model initially.

CALLAWAY: Was there a comparison of who won?
(Laughter)

PENCE: Well, my opinion is that we won.
(Laughter)
The problem arising with salinity, the reason I say I think

we won, is that the Corps of Engineers' model forecasted a much higher rate of intrusion than we did, and than actually occurred. I think the reason for this is that the Corps of Engineers' model arrived at a steady-state solution. They ran about 150 some-odd tidal cycles to get a steady-state solution, whereas in fact, the river never did arrive at a steady-state solution. We figured that with a 3000 cfs inflow, it would take it some 18 months to arrive at a steady-state, as far as salinity is concerned.

PRITCHARD: I am confused about that because I have heard Simmons describe the treatment. Now they just ran the river input. They didn't run a constant river flow.

PENCE: For the forecast, they ran a constant river flow.

PRITCHARD: I am talking about taking the actual river flow, putting the actual river flow for the year in and comparing the salinities observed in the model with those observed in nature.

PENCE: This wasn't what I was referring to. At the beginning of this salinity problem, the DRBC said "quick," you know, "somebody tell us what is going to happen." As I remember, there were three attacks. We modeled it with our digital program, the Corps of Engineers ran it on the physical model, and the Geological Survey did a regression analysis.

PRITCHARD: The way you described it, it suggested that the problem with the hydraulic model was perhaps improper input.

PENCE: Right. I don't mean to say that... let me take it back. If I gave the impression that I thought the hydraulic model itself was at fault here, I didn't. It was a situation where the big factor here was that they had to run it once and quit, and we got the chance to tippy-toe down to Washington every week, make two or three runs and change things, and what not.

THOMANN: I think that is an important point from an operational forecasting point of view. The physical hydraulic model is quite unwieldy and very, very expensive. You couldn't make forecasts every three or four days.

PRITCHARD: Yes, I think this is correct.

PENCE: We felt that our work at that time was quite valuable. It was used to regulate the flows. Well, I won't say it was used to regulate the flows, it was taken into consideration when the regulation decisions were made. And we did have the capability that the Delaware Basin Commission could call us up and say, "What will happen if...", you know, "if...", and we could run down to Washington and run it, and come back and say, "well, it looks like it will do thus-and-so."

PRITCHARD: Well, you certainly have an advantage if you've got, say, a tape with all the background information in the program, and all you want to do is update the predicted inflow. This can be done quite rapidly. While with the hydraulic model, you have to gear up and it takes half a day before the thing is ready to go.

RATTRAY: Well, you're predicting and comparing this with the cross-sectional mean salinity, aren't you?

PENCE: Not only the cross-sectional mean salinity, but the mean salinity in a section which can be ten to twenty thousand feet long.

RATTRAY: For a number of classes of estuaries, the longitudinal salinity distribution is basically governed by the one-dimensional model, although in the local salinity change the horizontal diffusion term is really a small part of the local salt balance. This means that whereas you can do a reasonably good job of cross-sectional mean salinity determinations with the one-dimensional model, other kinds of distributions that are not put in in the same way, essentially over a cross-section at the seaward end, but more local sources, are not going to be nearly as well described in the one-dimensional model. This is a real problem if you're talking about something other than salinity, or other than simple sources.

O'CONNOR: We can use an equation like this to clarify a point. That K includes a lot of garbage, a lot of things. You take the equations that you've worked with, and you lump all of these effects together into the diffusion.

RATTRAY: The point being that those different things you lump together are lumped together very differently for different river flows, or winds, and so on.

O'CONNOR: That's correct, and so you get the empirical correlations of that K with the Q plus the width, and so forth. And I think this is a very important point. We should compare this type of thing that you're talking about to the sum of the Q's, if I can relate that to DO or BOD or nutrients. Seeing the relative importance of these things is one of the more important things we have to do in an estuary. The relative importance of the hydraulics versus that of the kinetics of the reactions, and I, right now, am undecided as to how to pursue research on this point. This is one of the big questions we face. I realize that these both are going to go ahead simultaneously, but I would like to get some feeling for the relative weight of the kinetics versus the hydrodynamics.

RATTRAY: I guess what I'm saying though is this. With the cross-sectional mean salinity in a fairly wide range of coastal plain estuaries, the longitudinal eddy diffusivity, due to the tidal flow or whatever, in fact determines that salinity distribution, without all these other things being averaged in.

PRITCHARD: Not in East Coast estuaries, except at the upper end. In most cases the salinity distribution is really controlled by the vertical shear in the mean current and vertical mixing, and it comes out as an effective horizontal diffusivity, and surprisingly predictable by this one-dimensional equation, but it's only fortuitous. The K here is an effective horizontal diffusivity, not a true horizontal diffusivity.

RATTRAY: In a section through the middle of these estuaries, I thought the horizontal salinity gradient was about the same top and bottom in your estuary.

PRITCHARD: Right.

RATTRAY: And that horizontal salinity gradient times the eddy coefficient gives you the same salt flux, due to diffusion, in all layers.

PRITCHARD: Only if you use the effective horizontal diffusivity, not the true horizontal diffusivity.

RATTRAY: Well, I guess we'll have to discuss this.

PRITCHARD: There's another question I had, whether you had as a source or sink any biology?

PENCE: This is another thing we run into regularly. We have not included in any of our runs any photosynthesis as such. This is not because we choose to ignore it, it's because we ran a rather extensive testing program in the estuary and couldn't find anything significant.

* * *

ESPEY: Norbert Jaworski, from the Mid-Atlantic Region.

JAWORSKI: Being Polish always gives me the prerogative of telling a Polack joke. There was these two Polacks given the task of measuring the height of a flagpole. They tried everything; they tried to climb the ladder, even got the fire department out, but they couldn't get to the top of it. While they were doing this, a little kid came up to them and said, "What are you trying to do?" The two Poles said, "It's obvious, we're trying to measure the height of the flagpole." The kid said, "Why didn't you measure it two days ago when it was lying on the ground?" The Poles said, "We don't want the width, we want the height."

(Laughter)

What I'd like to do is tell you what we're doing in the Middle Atlantic Region, and I think George Pence gave you a pretty good short, quick looksee of what our activities are. Primarily, we're using the Thomann segmented steady-state and the Thomann time-dependent models. These are our two workhorses. These have been used on two estuaries, primarily, the Potomac and the James. Recently the Maryland Department of Water Resources took one of our guys, named Charles Hall, and he's going to apply it to the Patuxent.

So we'll have three basins in the Middle Atlantic region by the end of the year in which we have the model. Now the parameters which we simulate are primarily the DO and BOD. We have used it on phosphates. We have used it with some success on phosphates. We are currently using it, I am not quite sure how it's going to react, but I'm trying the ammonia to the nitrate system on the model. We also have simulated chlorides, primarily in the Potomac estuary. Dr. Hetling, who's with New York now, the question was posed to him, if D. C. used the Potomac estuary -- now you've got to realize the Potomac is tidal but is also fresh in the upper part of the system -- the question posed to him was can D. C. use the upper tidal system for a water supply. This is a salinity intrusion problem, and this has been worked out. As far as documentation of the models, I think George covered it pretty well. We do have the documentation on the chlorides model, as far as a pumping problem where you pump from one section and put the waste in another section. This gives you more flexibility than the original Thomann model. That has been fairly well documented.

As far as model verification goes, in the Potomac we have run a series of dye studies, and we currently are running more dye studies, in the lower part of the, middle part of the estuary. We have also verified it against salinity intrusion. And also, right now we've got a fairly extensive program going on in which we're trying to verify against the phosphates. The phosphates in the Potomac are a natural tracer, in that we are currently measuring what's coming in weekly -- we've got a sampling program going -- what's coming into the estuary from the fresh water system. We've got a hundred mile sampling program weekly in the estuary itself, on all forms of phosphates and nutrients, carbons and chlorophyll level. So we hope to, if we can use that same equation and put some type of decay system or deposition rate to the phosphates -- we have a natural tracer where we've got about 66,000 lbs./day coming out of the District of Columbia system -- so we hope to verify that model against the phosphate system. Now, in the James, we did dye releases, and also there is a physical model and Bob Horn knows more about that than I do, so if there's any discussion on the physical model verification against the prototype, Bob can talk on that.

As far as the limitations of the model, George went into them fairly well. I think there's one thing I would like to mention, and that has to do with the word stratification or cross-sectional variations. As Dr. O'Connor was mentioning, you can have a certain cross-sectional concentration of, let's say, phosphates in the system, but then if you look at the DO profile, it's completely different. And these driving forces, I think one will have to get a real good fix on. We have in the Potomac three biological systems. We've got the carbonaceous oxidation from the waste water. We've got the nitrogenous problem. And we've got the algal problem. O.k., now these three make up our BOD loadings into the system. The Potomac Enforcement Conference which was just held stipulated loads for carbon, nitrogen, and phosphorus. O.k., now I think the thing here is coming, the point coming is, and I think we've got to be very well addressed to it, that pollution no longer is the five day BOD. We're talking in terms of controlling aquatic growth, and I think we're almost in a different ball game, in that we're getting into things called nutrient transport. For example, on a high flow day in the Potomac, we'll get 200,000 lbs. of nitrates coming in over Great Falls. Where are these nitrates today if they came over in May? And this means that we've got to have the ability... if we're going to say we've got to remove nitrogen out of the D. C. system, that means either ammonia stripping or it means some type of biological denitrification. These are very expensive processes, so we'd better be damned sure that we know where our nitrates are and how our nitrates move in that estuary.

In the Potomac estuary where we call for a poundage loadings of phosphorus and a poundage loadings of nitrogen, for the control of eutrophication in the Potomac, the problem we're faced with is no longer a five day BOD, the simple plug in and getting an answer. I think we've got to start looking at things like species shifts. I've estimated that in the upper estuary we have more carbon being added to the system from algal production than any other source of BOD in the system, in the upper estuary. So I think if this is indeed the case, then we've got to start looking at this term eutrophication, and start modeling parameters which we can relate back to this algal production. And I think the nutrient transport mechanisms in the Potomac is one where we've got an extensive program going, and I hope by the end of the year to have some fix on the phosphorus system, phosphorus movement, and then I think the one which is a real kicker and the real sleeper is the nitrogen. Here we've got to be awful careful. We've got many compounds, we've got the organic, we've got the ammonia, and we combine these two.

$$\mathrm{Org} + NH_3 \longrightarrow NO_2 + NO_3$$

This one here is our direct oxidation, our second stage system, and I think, right how, we've got more BOD coming into our Potomac estuary from that than from the carbonaceous system. What hurts us is this oxidation process right here, and that as a nutrient. We may have a tremendous poundage coming in from this oxidation process, and we start doing cross sections on the lower estuary, and this completely disappears, that form. It's all being used by the algal cells and converted over to organic. And that's our culprit right now.

GALLAGHER: Are you developing that phosphate system as a linear-type relation?

JAWORSKI: Yeah, right.

GALLAGHER: What implications does it have as far as control is concerned?

JAWORSKI: Well, in the entire Potomac, something like 80% of our phosphates is from waste water systems.

GALLAGHER: O.k., but what is the base concentration of phosphates in the Potomac?

JAWORSKI: What do you mean base?

GALLAGHER: I mean what would it be if there were no point source inputs and so on.

JAWORSKI: That's a difficult question to answer, because we have a certain amount of fresh water input. Now the fresh water input right now is about 3/10 as PO_4, and 1/10 as P. Now that's a real odd one, too, because if you talk in terms of annual cycles, you get a tremendous flush in springtime, and then during the summer months, you get very low phosphorus coming over Great Falls, where you may have 18 to 20,000 pounds of phosphate being generated by the waste water plant in the upper system, but in the low flow months, you'll only get 1,000 pounds. So you get a tremendous amount of hangup of phosphates.

GALLAGHER: Let me ask the question more directly. Is it true, at least in this part of the country, that the base concentration of phosphates in sea water are chiefly above the limits which are considered to be healthy?

JAWORSKI: No, I'm not going to answer that question.
(Laughter)
You mean in the open ocean?

GALLAGHER: In sea water in general. What I am saying is that to model a phosphate system for control, to talk of phosphates in a control situation, is more academic than anything else, because you'll always have this base concentration that will support an algal population or a eutrophied population.

PRITCHARD: It's not a eutrophied population.

GALLAGHER: Eutrophication is a natural phenomenon. Accelerated eutrophication is something else again.

PRITCHARD: Well, I'm not sure I know what you're saying, but an ecological system in a marine environment is adapted to the nutrient levels that are naturally there. And to me, eutrophication is not a proper word here. Bio-stimulation is something else. You can bio-stimulate the system by increasing the nutrients to the point where you get undesirable organisms. But the levels which are naturally there support a healthy system. In the tidal river segment of what they call the estuary, in that segment of the Potomac which is fresh, the phosphates certainly are an important reason for the overproduction of undesirable organisms, which is not great. I consider a healthy system as one in which the phytoplankton population is maintained by grazers at a level which is on the order of one or two days production. If we go to the upper Chesapeake Bay, which I consider to be healthy, if you take out the zooplankton, you can get a doubling rate of the phytoplankton in one or two days, but we never see more than one or two days production there because the grazers are grazing. And if we go to the upper Potomac, the tidal river section of the Potomac, you have an organism which is not grazed, which blooms and dies off, and it certainly appears to be related to phosphorus.

GALLAGHER: I gather then from the consensus of your comments that you do believe that phosphates are not only a very significant factor, but may in some cases be the controlling factor in estuaries. That's what I wanted to know.

JAWORSKI: One reason why we're using the phosphates is that it is a beautiful natural tag. If you want a natural dye release, that is a beautiful one year round. As far as our other efforts go, we are going headway into nutrient transport. The question is always asked us, can we divert all the waste down to Maryland Point or 301 Bridge. That type of question is always asked to us. One of the things we've got to know is how those nutrients move through the estuary. And this brings up then the so-called K term, this diffusion term. We'll have to go on to some type of diffusion term for all flow systems. This is not a steady-state system any more. We will have to be able to model a dynamic system. In other words, we will have to go to a time-dependent model, I think, that will be adequate for this. But then our diffusion coefficient will have to be based...will have to be for all flows, and we're no longer talking low flow conditions. This is one problem that's introduced to us. And then we'll have to go, of course, to yearly simulations, and I think here a one-day simulation will be adequate with the Thomann model. But that's one of our big efforts right now.

Then our second big effort right now is in this so-called eutrophication modeling. This is the carbon balances, the organic nitrogen balances, and the phosphates balances. If you've got x number of milligrams per liter of phosphates or nitrates, what can you expect as far as carbon production, and again, what does this carbon mean to us in the DO balance? And this may require us possibly going to a two-dimensional time-varying system. If we need to go that far. I really don't know. We haven't made up our minds what we want to do with number two. But there's no doubt about it, in terms of the Potomac estuary, we're far into the nitrogen and carbon systems.

And then our next efforts, and these are real pain-in-the-necks, are small embayments. We've been asked a lot of times, what happens if we discharge an effluent into a little embayment, and these models won't help, they are too big for little embayment studies. And I think, if anything, I'd like to see efforts on really small embayments, a mile and a half long and a half mile wide. I think models for these little areas is one that should really be looked into. Simplified things too.

I've got three more things that I'd like to see as far as further efforts. First of all I'd like to see a glossary with definitions of terms.

(Laughter)

Diffusion coefficient, what the hell is it, I mean what the heck is it? Look, in the Thomann model it's a gross thing, and you look at Harleman's K and it's another thing. And I'd like to see, you know, when we're using it, in what base is it in? Is it base 10 or base e? This may sound trivial to you guys, but we guys who are using it, and if we have to spend three hours going back to how you derive this thing, we lose a lot of time. O.k. There are two other ones I'd like to see. Manuals. The Orlob model, I've never seen a manual on it. I'm not criticizing anybody. I think the Thomann model is fairly well documented. Maybe they're cookbooks. But engineers are cookbook followers. I'd like to see manuals laid out, what the units are in, how you use these things with sample cases. I can't stress this enough. One thing I like about Dr. O'Connor's work, every one of his papers, he's got a small example in it. I've got a guy from Howard University, I gave him all of O'Connor's work and said "go", and in two weeks he developed diffusion coefficients for the Potomac from our last dye study. And this is what I think we need. I think FWPCA ought to come out with users' manuals, and if anybody says "why aren't you using my model?" I say, "Let's see your documentation." I think this is one of our biggest criticisms.

QUESTION: What kind of documentation? When you say users' manual, what do you mean?

JAWORSKI: Nutsy-boltsy, boy. You betcha. If it isn't done, no matter how nice those equations look on the board, if we don't have a manual to use that thing, that model's worthless to us. And we've got to make the decisions at the regions on what the treatment requirements have got to be.

And then, another gripe I've got, if this is a gripe session, is I'd like to see another manual, on dye releases and the interpretation of dye release data, on continuous or batch releases. I think there is a real lack. Everybody's got their little pet projects on dye studies. How do you really analyze dye studies? Dye studies have been made ill-planned, I've made them ill-planned, and I've analyzed them ill-planned, and I think others have done the same thing. You get in the field and you dump the dye. I call up Carpenter and I say I can't measure it. Carpenter says you're using the wrong fluorometer.

(Laughter)

This happened, to me, and I'm sure it's happened to everybody else. I think if we're going to get into modeling, we'd better get good common techniques on dye interpretation and dye release data. This is all I've got.

O'CONNOR: That's a fair amount.

(Laughter)

I quite agree that this is the next level of work we should be getting into, and I think we have to start on the basis of saying that, in general, these are all highly coupled systems. You have the dynamics of the bacteria, the carbonaceous, the nitrogenous, the algae, and I think this is where we're going to start to make the next significant inroads in modeling. These are very complex systems and I think we should start gathering data now, the implications of this being, as I see it, if you're going to Congress to ask them for money for data collection, boy, you'd better buck up by orders what we've been doing previously, in order to tie down these things.

JAWORSKI: For example, in our estuary runs right now we're measuring three forms of phosphorus, four forms of nitrogen, total organic carbon, and the five day BOD, chlorophyll, and right now we've been hit with a real hooker: CO_2.

O'CONNOR: Yes.

THOMANN: I think this brings up the point that we were talking about before on digital modeling versus analog modeling. What's required here, and I don't think we should underestimate the order of the kind of models we're talking about, as far as nutrient recycling is concerned. There we're talking about new conceptualizations of the models that we require to answer the kinds of questions you're raising, and include such phenomena as feedback, for example, non-linearity, the factor of nutrient limitations, non-linear reaction terms, and so on. That's what we mean by new types of models. And we'll just have to rely again on digital, analog, and whatever techniques we have available for solution. But I don't think we should underestimate the order of the kinds of models.

JAWORSKI: The data you're asking for. Boy, you're asking for the verification of nitrogen cycles in the field, and you're talking about compilation of data in the field that you've never seen.

HARDER: What are the significant interactions among these variables that you are measuring, aside from those that are related, of course, by chemical equations? For instance, do you find phosphate and nitrate interacting in some fashion in algae production?

JAWORSKI: I ran a series of regression studies, and you can get what you want out of regression studies. I wouldn't want to answer that question. I ran regressions for segmented parts of the Potomac data, and I threw in the Chesapeake data, and I threw in the James data, and the regressions show you practically anything you really want.

PRITCHARD: The problem here is that the turnover rate is so different for phosphorus and nitrogen. The evidence in the upper Chesapeake Bay is that phosphorus is turning over once every day.

O'CONNOR: What do you mean by turnover, Don?

PRITCHARD: I mean that it goes into the plankton, out of the plankton, and back into the plankton, through the herbivors and their excreta, and rapid oxidation, and the nitrogen doesn't act this way at all.

JAWORSKI: We found the same thing in the Potomac estuary where we do mass balances throughout the estuary. The phosphorus levels all stay up like Dr. Pritchard says, but this one here, the organic nitrogen is really a ...

PRITCHARD: And you can't really get any pattern of phosphorus. It's up and down, but it's just cycling, and maybe there's some loss at the bottom. For nitrogen, most of the nitrogen in the upper bay system comes down the river in winter and spring, so we begin in June, say, with a concentration in the bay of 20 microgram atoms per liter, and by September it's down to 4/10.

O'CONNOR: That's another thing about terminology, I think. We're so used to milligrams per liter that I have to pause and make a conversation.
(Laughter)

PRITCHARD: But there, you see, it's just a steady loss into the bottom, and it doesn't regenerate. The next big river flow or the next winter brings in a new supply of nitrogen.

O'CONNOR: I think this is an important point, too. Bob mentioned earlier that we're going to discuss scales. This is probably a good time to bring one point up. We're talking about, perhaps, two different time scale phenomena. One of them is seasonal changes on the order of months, two month periods, which affect large masses and bodies of water. And in such cases, at least we people can make some simplified assumptions with respect to the flux conditions, because these are such larger completely-mixed systems. And then as we go down to the smaller terms, the diurnal changes, etc., then we're back into the more complex situation, and even the more complex hydraulic model. But I think when we look at the kinetics of these overall systems, they relate to spatial and time scales, and I think progress along those lines should be balanced.

THOMANN: I just want to comment also on one of your remarks. The models that you have now are too big, did you say?

JAWORSKI: No, did I say that?

THOMANN: When you were referring to bays, you said, the models were too big.

JAWORSKI: Oh, yes. What I meant there was the scale. Let's say you've got a small embayment in which the tidal excursion is the whole embayment, how do you really model that thing? Sure, you can put more sections in there, but well,...

THOMANN: My point here is that if you accept those kinds of equations that are written there...

JAWORSKI: Right. But I'm saying is that, suppose you put a discharge here, you may have a real localized problem.

Here is a matter of scale again. You may want to investigate the small area, just to get a localized effect, about two miles. And these models will not give you that answer. If you are discharging into a big bay, you really don't care, but in a small embayment, when you've got to make some real finite decisions on how this pattern really should be, say somebody has got a real rich home over here, you want to be damn sure your answers are right.

(Laughter)

The present approach is too gross, too big. It's just a matter of scale.

THOMANN: If you accept those equations, either in a one-, a two-, or a three-dimensional form, you know...

JAWORSKI: I go with that. I know you can get all of this. But the amount of money to solve this little 5 MGD plant problem, which are pains in the neck and you get them all the time, the money to verify the model is going to be more than the whole sewage plant cost us. So a nice little, quick and dirty answer to a small embayment problem. This is what I'm really after. A real simplified solution if there's anything like that for a small embayment. Because there's nothing worse than putting a 5 MGD plant in a dry stream, and this is the same type of analysis.

O'CONNOR: I think that the model that George talked about before might be helpful in cases like this, that is, on a steady-state one-dimensional basis. You have a little more flexibility using the analytical solution and making constructions from that. And I think that type problem lends itself very nicely to the third model George spoke of. Conceptually you can include it very readily on a smaller space grid, and then you use a different computational technique to get at it.

GALLAGHER: But isn't it true the amount of data needed to verify that sort of thing goes up exponentially as well. And what for, a 5 MGD plant?

O'CONNOR: Then don't bother.

GALLAGHER: But he wants to answer the question.

JAWORSKI: Maybe we don't have to go to a model. Maybe the idea of good dye studies techniques would help, sort of a conventional analysis. But this is a problem we are faced with all the time. Big discharges into big water is no problem. Big discharges into little water give us problems. And the're real problems, too, boy.

PRITCHARD: One problem with a small embayment, at least off the estuary proper, is that the controlling terms, say for the flushing and exchange of water, are different from the main body of water. You can readily write the relationship and solve it by the various means available to you. But it's a different relationship. Actually these small embayments off the tributary to an estuary are primarily controlled by the time rate of change of salt concentration off their months.

O'CONNOR: I don't want to overemphasize this, but I don't appreciate the difficulty of this. I think that it could be readily be included in the existing programs. And there's another thing, you were emphasizing cookbooks before. I very much appreciate your need for it and why you want it, but because we're dealing with natural phenomena in different parts of the country with different geometries, you're not going to get that sealed envelope thing, you know.

JAWORSKI: No, but I think for applications of a model, you ought to have a cookbook.

GALLAGHER: You've got to give us a little credit for having the patience to adapt it to the contingencies of local situations. But I think the escalation of jargon has got to the point now that the user has problems.

JAWORSKI: If it takes you two or three days to just get down all the terminology and what all of the coefficients mean, then you've lost the utility of the model. Sure, you have to dig into a lot, but to get the theory down is one thing, but if you spend days going through that thing learning how to code the damn thing up, then it's not documented right. There's no two ways about it.

RATTRAY: I want to point out the other side of this. There is a danger in getting it into a cook book because it sort of gets inflexible and you believe that it is the bible, and I think, in fact, that a lot of people have done this, don't feel that we're that far along yet. And that's why we're all here essentially, to try to do better. Let's not get something so cook book rigid that everybody uses it ad infinitum and people forget that maybe it's not as good as it might be.

WASTLER: This is a very real problem. Some of the higher levels of administration feel that because the IBM 360 has pretty flashing lights you can believe everything it prints out. And getting things to the cook book stage sort of reinforces that. Once it's written down, you're stuck with it.

GALLAGHER: Maybe the problem is semantics. We shouldn't use a word like cook book. We should use a word like guidelines.

JAWROSKI: Well, I think the documentation of the time-dependent model is a good documentation.

THOMANN: What you're referring to there is a write-up on the program.

JAWORSKI: Right. O.k.

* * *

ESPEY: Well, let's move along to the next speaker. South Central. Mr. Kallus.

KALLUS: Well, in the South Central region, we cover quite a bit of the Gulf Coast, but FWPCA per se is not doing any modeling. There are two modeling efforts going on, both in the same bay, Galveston Bay. TRACOR is doing one for the State of Texas. We have a grant with Texas A&M Research Foundation to do another one. I'm not going to presume to talk about TRACOR's model here. I did ask Dr.Hann to answer the specific questions that Dick Callaway sent out, and he gave me some answers which I will read to you. The first question was on the general nature of the model, and he says his is a deterministic digital finite-difference model designed to handle a primary pollutant and secondary water quality indicators such as BOD and DO. He says the model will handle estuaries varying to homogeneous to highly stratified. It can handle other conservative and non-conservative parameters where a reaction rate and so forth can be specified. Modeling effort is geared to begin with a steady-state model and then proceed to dynamic modeling using "short time intervals." The models are being verified by extensive sampling programs and to date documentation consists of a draft on a one-dimensional steady-state model.

The model is expected to be flexible and applicable to most Gulf Coast estuaries. For my own part, I feel that this model, when it is developed, can be used by at least one other region, Southeast region, on the Gulf Coast. As far as is known, it appears that the South Central region has no plans for further estuarine modeling at this time. I would like to reinforce the statement Jaworski said about coming up with something else than BOD for modeling, because we've been having one hell of a time in the Houston Ship Channel getting any BOD values that mean anything.

And that's about it for the South Central region, unless Chris Shilling or anyone else would like to say anything.

SHILLING: I might add just a couple of comments. In the first place, I don't know whether we have any true estuaries, what you people talk about as being estuaries on the Gulf Coast or in the South Central region, with the exception of possibly the Mississippi River. The rest of them down there are what you might call embayments or bays behind barrier islands, a maximum of about 15 feet deep. So when we talk about modeling those things down there, we're not talking about in terms of this stratified model, and so on. But we are concerned very strongly with the effects of wind for one thing. We have a very small tide down there, usually about a foot or a foot-and-a-half. Occasionally you'll have a seven footer which will swamp out all the oyster houses. So I'm just a little bit leery of trying to apply some of the models, some of the formulations and such, that have been developed on the deep estuaries with pretty good-sized river inflows. And I'm not sure I know what a steady-state condition is. Certainly, one of our steady-state conditions may be steady-state for about six hours or a day, but in most of those bays down there, there are changes overnight, perhaps the wind is blowing generally southeasterly and becomes a strong north wind a very few hours later. This changes the conditions of the bay quite considerably.

I did just want to make those brief comments, so that when you are doing work along this line, I'm sure TRACOR and Hann are taking this into consideration, that not all of this is directed simply towards deep estuarine areas like New York Harbor or the Columbia River or some of these.

The other big problem that we have down in that area is the cutting off of fresh water inflow. Now this is very true in Texas on some rivers at the present time, and it's going to become more true. And the other problem of course that we get into, if the so-called Texas Water Plan goes through, where they start diverting water out of Louisiana, then we have the question of what is the diversion of inflow going to do to the estuarine areas, so this may get into your biological modeling and a few other things.

* * *

ESPEY: Any other comments? And to answer your question, we are considering those factors. TRACOR, as you mentioned, is working under contract to the Texas Water Quality Board, and working very closely with Mr. Kallus and FWPCA to develop these models for the Galveston Bay system. And one of the important mechanisms that we feel has a very dominating effect is the winds in this shallow estuary. The Water Quality Board has submitted a grant application to FWPCA along those lines.

The next speaker representing the Southeast region, Tom Gallagher.

GALLAGHER: The last two speakers have almost stolen all the thunder that I was going to talk about here. I can't agree with Dr. Jaworski any more. It seems like we set it up before we came in here what we were going to say. I think what I will probably say is generally a series of war stories,

(Laughter)

which is our experience in the Southeast. We got started a little later than most of the other regions because the Technical Programs Operations in the Southeast started just about two years ago, so we had the privilege of using the benefits derived from the other regions in terms of developing these models and forecasts. In general, we've had to use simulation models on small projects, the enforcement-type projects. We've had a great deal of success in using the material put forward in that "Estuarine Distribution of Non-Conservative Substances," -- is that the name of the paper? -- that Dr. O'Connor put out four years ago or so. It's been very successful for the quick and dirty jobs that are necessary to beat a city or industry over the head in terms of getting abatement or control in the short term.

Two years ago, the mayor of Tampa...well, let me explain the situation with Hillsborough Bay. We've had a large project in Hillsborough Bay for the last two years. We have a floating laboratory which is colloquially known as the Barge in the region, that was transferred from Charleston Harbor to Hillsborough Bay on the request of the mayor of Tampa and certain of the commanding generals at MacDill Air Force Base, because of an odor problem that was occurring along the western shore of Hillsborough Bay. This odor problem was occurring in an area of expensive houses. So having heard the voice of the people,

(Laughter)

the administration decided to commit some money to see what could be done about this problem. It turned out that these odors were occurring... although there was no quantitative reporting of these odors, the qualitative experience were that they were relatively random and couldn't be related to any naturally occurring phenomena, at least they couldn't superficially be related to any naturally occurring phenomena or possibly to waste discharges. It was the consensus of informed speculation about that time that it was due to the death and decay of a weed, an attached algae called Gracilaria, that grew in great profusion along the western shore, and that this death and decay was triggered by the discharge of twenty-five to thirty million gallons per day of primary-treated sewage from the city of Tampa, which was discharged to the center of the bay. After fooling around for a whole year trying to relate it to DO-BOD situations, we concluded, as a result of both laboratory and field studies, that this situation was the result of an unbalanced ecological system caused by the discharge of nitrogen into the bay from several point sources, particularly from the city of Tampa water supply reservoir, which is up the Hillsborough River on a tributary. They have a water hyacinth problem in Florida which you may or may not know about. Water hyacinths have something like 16% nitrogen by weight. When they get thick enough in a reservoir that you can walk on them, they're sprayed and then released, and they come into the bay, Hillsborough Bay in this particular situation, where they deposit, drop down and subsequently the nitrogen, I gather, is taken up. This becomes a very significant source of nitrogen to the bay, something like 700,000 pounds with each dumping. In addition, on one other tributary, there are 14 phosphate processing plants, and the bay receives upwards of 150,000 pounds per day of phosphate, total P. The phosphate concentrations in the bay on the average are about three to six parts per million all the time. The nitrogen concentrations vary depending upon the season. You may get no nitrate at all at certain times, you may get high concentrations at other times, usually in the winter. As it turned out, this Gracilaria or attached weed was about 98% of the attached algae population in this bay, indicating a severely unbalanced system. And what was causing the death and decay of the algae was that it was salinity sensitive, and that it was short circuited down the western shore from discharges of fresh water from the tributaries. This was causing the odor.

One of the objectives of the project was to evaluate means of abating or eliminating the odor, and we felt therefore that we had to verify this thing from a salinity viewpoint to have some quantitative hold on what should be done with respect to either diversion, or holding back of fresh water, or changing the physical system. We used the segmented model that everybody on the east coast has used, and we made it quasi-two-dimensional--is that the word for it?--quasi-two-dimensional, by drawing a grid pattern on Hillsborough Bay, and solving it the same way through a matrix inversion, and we got some fairly good results and verification. (That's an interesting word, verification. It sort of implies the truth is what you say it is, which is an interesting philosophical concept in itself.) But, anyway, we got some pretty good verification on the dispersion coefficients by verifying against the three month average when there was hardly any fresh water inflow into the bay. And then we used those dispersion coefficients as constants and went to a higher inflow into the bay of fresh water and supposedly verified the advective situation as well. And we were fairly successful, I think.

We made some recommendations with respect to control and abatement programs, management programs for the Bay.

The important point to make here is this was not a DO-BOD problem. And, at least in my experience in the Southeast, we don't have DO-BOD problems in most of our waters. We have coastal waters and we have bays, as Mr. Shilling said. And in these situations, your eutrophication or accelerated eutrophication, bio-stimulation, becomes a much more significant factor than carbonaceous BOD demands, nitrogenous BOD demands, whatever you may want to call it. On a gross basis in Hillsborough Bay, over 80% of the demand is due to the algae respiration, 12% to point sources of waste, and about 4% to benthic demand. Now if this is the case, it would seem that to refine carbonaceous point-source loads, to refine and sophisticate nitrogenous point-source loads would seem to be refining and sophisticating the tip of an iceberg. The most significant factor in this case is your algal photosynthesis and respiration, and it is in most of the bays of the Southeast. And I can only endorse too strongly Dr. Thomann's statement that what we need is new conceptualizations here. We are talking about including the algal function. We are talking therefore about nonlinear responses. We're not talking about superposition of linear responses in DO and BOD in the classical sense.

This also has implications for control programs. In Hillsborough Bay we recommended secondary treatment because it happens to be a constraint to the administration, but in fact it had nearly nothing to do with the immediate problem. The problem was one of a salinity-sensitive system that was just grossly polluted because of excessive nutrient concentrations.

Well, that project has been completed. The report is undergoing internal review within the administration.

The people who were on the project have now been moved to Fort Lauderdale at the request of the governor. He has a home on Lake Worth, and Lake Worth happens to be severely eutrophied. Severely? Yeah, severely eutrophied. It's green.

(Laughter)

The governor has requested a study, the mayors of several cities down there have requested a study. We have been directed to come up with, at least within our region, we have been directed to come up with a water quality management plan for the area on the Florida east coast from Lake Worth to south Biscayne Bay. The problems that are involved here, again are not straight point-source waste problems. They are dredging and filling problems, how they affect flushing and exchange, in other words, physical alteration of your system. They are problems of dumping fresh water from flood control canals, into the Intracoastal Waterway which is a relatively confined system, how this affects eutrophication because they have severe nutrient loads. Let's see, we managed to get seven problems out of this thing. The ocean outfalls are becoming a very big thing. There are two demonstration projects now underway in that area down there to evaluate the effects of ocean outfalls. Thermal pollution. I'm sure you're all familiar with the Florida Power & Light situation on south Biscayne Bay. This is a nuclear power plant that's going to use four billion gallons per day of cooling water. The immediate objective of the project is to come up with a required heat input into Biscayne Bay from this plant. And then finally, of course, the eutrophication problems in Lake Worth and just plain sewage disposal into the canals as a result of the mushrooming development in and around Fort Lauderdale; Hollywood, Florida; and Miami itself.

Let me summarize and generalize, it's always sort of fatal to generalize, but I would say that our problems relate to data aquisition, in quantity, how much do we need, where do we need it, what are the most significant factors, and how can we evaluate these things before we have to commit a large-scale field operation, because that costs a lot of money. We estimate that to put a man in the field to do estuarine studies costs about $30,000 a year. That's generally about twice as much as a fresh water type study, and that's the government. You can imagine what it costs in a private-type situation.

Definition of problems. I don't think that our problems any more are point-sources of wastes, DO-BOD problems. They are new conceptual-type problems that involve biological responses that are generally non-linear, and they are going to require a whole new look at the way we do things. Third, is in terms of methodology. Most of the things that we have used up till now have been steady state, they've been one-dimensional or two. They can be adapted to two and three-dimensional obviously. But we need some indication really of what is an effective time response to use. What sort of optimum data requirement are there in both time and space in your system, so that you're not collecting data that's relatively useless to you. I think, in this area, when you start talking about residual pollution or some areas like this, the idea of optimization and linear programming has some real applications, not so much for optimization, because given political and social and economic constraints, I don't think you're ever going to operate under an optimal technical condition, but the marginal strategies that you can get out of this can indicate to you what would be the effects, say, of marginal pollution, residual pollution, this sort of thing. I think there is an application there. And the fifth thing, of course, as Dr. Jaworski said, is communication. I don't think we would have been able to do what we did do in Hillsborough Bay without Dr. Thomann's help, and without that "cookbook" which the Chesapeake Bay Project put out with respect to the segmented model. And that's about all I have. Are there any questions?

PRITCHARD: This odor problem you had, this was related primarily to an attached plant?

GALLAGHER: Yes.

PRITCHARD: And this plant grew in fresh water or salt water, I didn't understand?

GALLAGHER: Salt.

PRITCHARD: So that when the fresh water came by, the drop in salinity killed them, and it was the dominant oxygen producer?

GALLAGHER: No, phytoplankton was.

PRITCHARD: Then you said that 80% of your BOD, this was dominated by the phytoplankton and not by this attached plant?

GALLAGHER: Yes. The attached plant was only related to the odor.

PRITCHARD: And you associated with the fresh water releases a large amount of nigrogen input?

GALLAGHER: Yes.

PRITCHARD: Now this attached plant, when it starts growing again, is its production primarily related to high nitrogen?

GALLAGHER: No. That's right, I did not distinguish here really. The nitrogen concentration I think is what is causing the algal blooms.

PRITCHARD: Not the added phosphorus?

GALLAGHER: No. As a matter of fact, as a result of laboratory tests and field tests, it turns out that this attached algae actually thrives in degraded water conditions. Apparently it has reached an optimal growth condition of its own, with the particular high phosphate and high nitrogen concentrations that we get at particular times in this estuary, and nothing else really survives. It just takes over.

PRITCHARD: The attached algae?

GALLAGHER: The attached algae. We really don't know, but what we think happens is that everything else becomes suppressed. It just happens that this particular species thrives in this sort of environment.

PRITCHARD: But during the time when there is somewhat less but still high both phosphorus and nitrogen, the phytoplankton dominate total productivity and utilization.

GALLAGHER: Yes, the photosynthesis and respiration, in terms of demand.

BAUMGARTNER: Are you going to issue some kind of report or documentation, user's manual, on how you adapted Thomann's model to this two-dimensional configuration?

GALLAGHER: Yeah, sure. The technical director of this project, who couldn't be here today, could discuss this in much greater detail than I can. He did all the programming. But he intends to do that.

CALLAWAY: It is laid out in a grid, huh?

GALLAGHER: Yes.

THOMANN: There's a minor technical point here. In the distinction between what you were calling quasi two-dimensional and this, which is two-dimensional. The word "quasi" perhaps is not a good one.

GALLAGHER: I'm sorry. You're right. It is a two-dimensional type of situation.

CALLAWAY: Then it's not your original model.

GALLAGHER: Yes, it is.

THOMANN: Which original model?

CALLAWAY: The DECS model.

THOMANN: The original model was multi-dimensional.

O'CONNOR: Let's get back to the distinction we talked about before. When you put names on, with your first remark it was his original model, one-dimensional and so on, now you're talking about the two-dimensional.

GALLAGHER: Well, really it's only a procedural thing, not a conceptual thing. All you did was add four or five more terms to take care of the lateral situation.

O'CONNOR: Well, don't put the qualified adjective "only" onto it. (Laughter)
It is a two dimensional model.

GALLAGHER: It is a two-dimensional model.

O'CONNOR: The K_x and the K_y, the u and the v velocity. Tom, I certainly subscribe to your major points, particularly with respect to the problems you meet in that part of the country. However, by looking at them, don't de-emphasize, if in your part of the country BOD is not significant, in other parts of the country it is. I think the important point here is that once we see a new problem come up, don't de-emphasize the old. The important point is to keep the total picture in mind, which in most areas is both a bacterial and an algal problem. As we move from one to the other, I think this is precisely the problem we're going to have. As you clean up the bacteria situation, you'll move into an algal one. As as you clean up the algal one, I'll give you ten to one you'll move back to a bacterial one. And this total cycling that you've sketched up on the board there is precisely the thing we get into. Including the point BOD sources.

GALLAGHER: Yeah, well, maybe in that sense you do. The impression I got this morning, what most people seem to be doing is concentrating on expanded Streeter-Phelps models, more or less, that are dealing with a point source of DO-BOD, and coming up with what the consequences are going to be if you change that point source or you move it, or something like this. Now, it seems to me that as a matter of policy, that certainly the FWPCA and presumably the states definitely have made a commitment to getting rid of this stuff, carbonaceous and nitrogenous waste. Well, they're on the point, I guess, of making a commitment of getting rid of the nitrogenous waste. Whether they do back it up in terms of money is something else again. And, therefore, to put a lot of resources into refining this sort of thing, when really the most significant factor, at least in our own area, and I would assume that you've had a lot of controversy in New York Harbor over this too, is the biological system.

O'CONNOR: The BOD is part of the biological system.

GALLAGHER: Yeah, well,but it's not treated that way.

O'CONNOR: Of course it is, and that's why you're making this distinction, like the bacteria really aren't a biological system and the algae are.

GALLAGHER: All right, all right.

O'CONNOR: You just can't lump them in together with the BOD. But it is equally a biological system, and equally nonlinear. We simply have more of a background, an experimental and empirical background, to...

GALLAGHER: O.k. then you're making the point really then that we should actually look at this in terms of the way it actually operates instead of the way we have been doing it, and refining K's, my god...

O'CONNOR: Well, I would give you ten to one that as engineers we are going to get back to a different set of K's with the algae.

GALLAGHER: Yeah, probably.

O'CONNOR: And that's how we're going to answer the problems. We're going to be carrying on the same old problems with us as we go.

GALLAGHER: Yeah, that's right. That's why we need biologists, by the way. Yeah, this is interesting you know. Is anyone a biologist here? What we have now with biologists is that they just happen to be too qualitative and certainly too specific. They won't extrapolate and they won't quantify their data. It's terrible. And these guys can really help us out, they really can. Or we can help them out, one or the other.

BAUMGARTNER: Dr. Paulik, who isn't here, I'm sure he'd like to say something in answer to your last question, and he may when he reviews the transcript.
(Laughter)
With this model were you able to evaluate alternative remedies such as flow diversion?

GALLAGHER: Yes.

BAUMGARTNER: Did that show any significant...?

GALLAGHER: Yes, it did, because they were short-circuiting the system. If they were not, if it were a completely mixed system, I doubt that it would have. But because of the low flows involved, I think the maximum flow you can expect out of this river is only four or five thousand second feet.

PRITCHARD: I gather you have systems along there which have varying amounts of nutrient inputs and varying amounts of nutrient concentrations. I'm sort of going back to a question you asked when you were in the audience. Is it your impression that some of the systems which have relatively high nutrient content, that are salt systems, when there is salt with a given level of phosphorus nutrients and nitrogen nutrients, they are not undesirably bio-stimulated to the degree that a fresh water system might be with the same concentration?

GALLAGHER: No, they are less responsive, is that what you mean?

PRITCHARD: They are less responsive?

GALLAGHER: They seem to be.

PRITCHARD: This is an impression that I've had to come out. I know that **Bremer** has looked at several systems, the James itself, and tributaries to the James, which are loaded, and he finds undesirable bio-stimulation in the fresh water parts of the waterway, most of these are tidal waterways, and not in the salt water part.

GALLAGHER: This is something we have observed too.

PRITCHARD: I think that this is a matter of level. I think you can get undesirable bio-stimulation, well, you have had in this bay you just mentioned.

GALLAGHER: Oh, sure, yeah. I think that another comment to make is that the Gulf Coast is entirely new to us, and the

South Central people made the same comment. It seems like it's horribly complicated. The current and diffusivity and so on. A maximum current of something like 3/10 of a knot, and you get a 24 hour cycle, it bounces up and down and could be flood or ebb, depending upon how the moon feels that day, or something. It's a terribly hard thing to do when you start talking about time response to try to get it down to less than a day, and I don't know why we'd want to. I'd like to get one aside. When we started this Fort Lauderdale operation, we wanted to call this the Florida East Coast Estuary Study, but the regional director didn't agree with the acronym, so it became the Lower Florida Estuary Study.

(Laughter)

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ESPEY: We're going to start our lunch at 12:00, so I suggest we break here and wander over to the dining hall for lunch and continue with the regional discussions after lunch.

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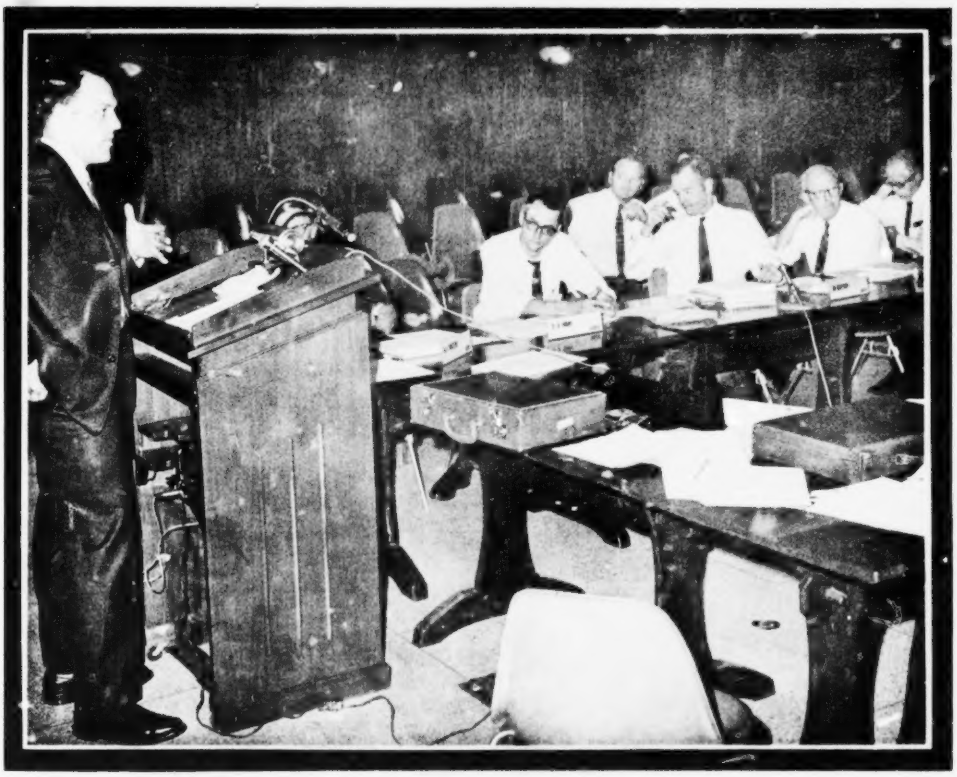

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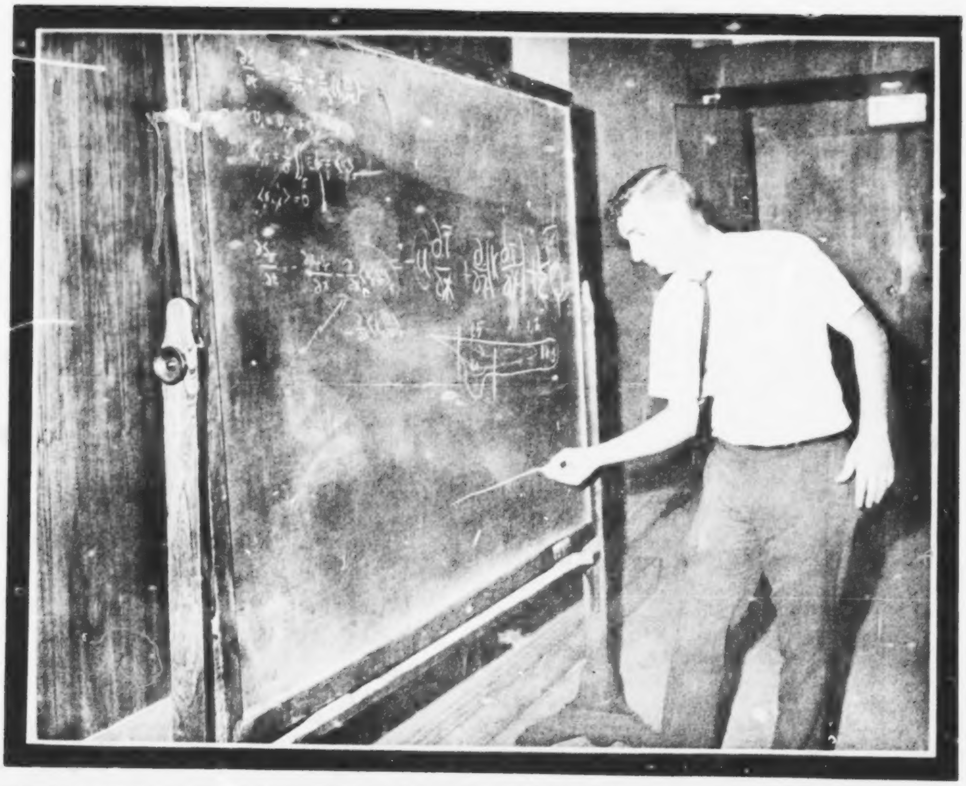

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5. AFTERNOON SESSION

ESPEY: Ken Feigner, representing the Southwest Region.

FEIGNER: The model that I'm going to be discussing today will be the same one essentially as described earlier by Dick Callaway. This model was originally developed for a really rather limited and unique section of the San Francisco Bay and Sacramento-San Joaquin Delta area.

The system that I'm talking about, originally the model was developed just for the fairly well-defined, in terms of channels, area of the Sacramento River, San Joaquin River going in from the south, and all of the interconnected maze of channels, these are well-defined channels protected from flooding by levies usually on both sides of the channels. I'm sure as Dr. Harder can attest, this is an extremely complex hydraulic regime, and the problems associated with modeling of course are equally complex. The early attempts for development of the model were somewhat along the lines of Dr. Thomann's approach, in that a steady-state type model was originally developed. However, the problems associated with coming up with a net tidal flow in all of these channel elements was something that was completely overwhelming. You can perhaps appreciate the fact that on flood tide perhaps 100,000 cfs flows upstream, on ebb tide 103,000 cfs flows downstream. It's essentially physically impossible to accurately measure these, to integrate over the full tidal cycle, to come up with a mean tidal cycle flow. So there was this inherent problem in this rather unique hydraulic system which would have precluded the use of a steady-state type model in this respect. This, by the way, was one of the primary reasons for having to go to the very short integration time step for solving this system. Some of these channels are only on the order of a few feet long, and yet they carry a significant tidal flow, so it was necessary to include these in the mathematical model. And due to the instability that Dick Callaway touched on, due to the wave celerity propagation through the system, the numerical stability requirements dictated a very short time interval. For the original model, this was on the order of 2 1/2 minutes. This model, as Dick mentioned, was developed by Dr. Orlob, through contract arrangements with FWPCA. After what we considered a successful application to this portion of the system, we then recontracted with Dr. Orlob to extend the model to include the Suisun Bay portion of San Francisco Bay and estuary and the San Pablo Bay portion. As already has been pointed out, the equation of motion and the continuity equation were used to describe the hydraulics in the fairly well-defined one-dimensional hydraulic system of the delta, and rather than go to the strictly two-dimensional approach, the two-dimensional network approach was extended into the embayment system, and the one-dimensional forms of the equations were applied to the two-dimensional network. The success of this was indicated by favorable comparisons of model, primarily tidal stage,predictions with time over a full tidal cycle with measured tidal stage fluctuations in the prototype. And this successful extension did indicate that the one-dimensional form of the equation could be applied to a two-dimensional hydraulic system.

Most of my comments are going to be directed specifically at the version of the model that was utilized by FWPCA, or has been utilized over the last three or four years. Additional development by Dr. Orlob has been going on over the past two years for the State of California Department of Water Resources, and perhaps at the end, I might touch on some of the additional work that he has been doing. Basically, the model is a two-component model, the first component being the hydraulic component, which describes for every channel element or link in the system, the velocity for each, say 2 1/2 minute time interval over the full tidal cycle, and in addition for every node or junction, the connecting nodes, describes the changing water surface elevation over the full tidal cycle, again for each time interval, 2 1/2 minute time interval. I think that it's recognized that this very short time interval is not really necessary for most water quality predictions, where we probably are at the most interested in a mean tidal cycle concentration, or perhaps a maximum or minimum concentration over the tidal cycle. But, as I mentioned, this short time interval at least for the hydraulic portion was primarily dictated by the geometry of the system and the stability criterion that Dick mentioned earlier.

Since we've already talked about the stability, I'll skip over part of this. I don't think Dick touched too much on what he actually meant by a dynamic steady-state or a steady-state hydraulic condition. Certain boundary conditions are imposed on the model, this would include fresh water inflows to the system, a specified tidal fluctuation at the seaward boundary of the system, and in this case, it was at the lower reaches of San Pablo Bay. We did not at that time have the model completely extended to the ocean. In addition, water that was exported from the system to Southern California, additional water uses within the system, were included. Given these hydraulic conditions, the model was run or solved on a digital computer. The solution continued until the net flow in each channel reached a steady-state value over the full tidal cycle. In other words, in each channel the integrated velocity inflow over the full tidal cycle reached a steady-state value. It would be typically reach, say, when the heads at each junction repeated themselves within approximately 1/1000 of a foot, at each tidal cycle interval, so even though this type of accuracy couldn't be measured in the field, at least numerically, this was what was required to come to a so'called dynamic steady-state situation.

The output from this hydraulic portion of the model was then used as one of the basic inputs to the separate quality components. The other critical input to this, of course, would be the waste loads of the system, the specified quality background concentrations, the starting concentrations in the system, and again the same, or identical hydraulic conditions were then repeated for as long a period as was desired in order to simulate the behavior of the system. Provision is included in this model to account for the effects of fresh water inflow, wastes that are exported from the system in any waters that are exported, water use and reuse within the system. This entire area is used heavily for agriculture, so that there is a water use and reuse factor which again complicates the quality picture. The model also includes the effects on quality of evaporation. Each of the nodes, or channel-connecting nodes in the sytem, is capable of handling a hydraulic input so that if desired a small hydraulic withdrawal could be included at each junction for evaporation. The model can handle up to 5 constituents simultaneously, including conservative and non-conservative constituents, and also including the interrelationships between BOD and DO. It can also handle a non-conservative constituent which might not be linked with some other constituent such as a radionuclide, which would just decay but would not be tied in with some other constituent. As opposed to the 2 1/2 minute time interval, the basic time interval utilized in the quality portion, or the quality component, was on the order of one-half hour. The hydraulic inputs were thus averaged over the full tidal cycle in half hour steps or increments, so that the basic hudraulic input then became a mean velocity for each channel element over the full half hour time period.

The output from this model is quite flexible, and includes the time history of each constituent considered for the entire duration of the run. We used the model for basically two different types of water quality studies, those involving steady-state type predictions of quality which would result from a given set of hydraulic inputs and a given set of quality waste loads. The second type of study that we conducted with the model was more concerned with the rate of change of concentration of constituents, as opposed to a steady-state type prediction. For example, of extreme interest in this system is the rate of salinity incursion into the system and the rate of salinity repulsion in the system whenever there is a storm runoff. These rate type studies are particularly of concern in that with the California Water Plan of the State of California and Central Valley Project of the Bureau of Reclamation, their future projections of water being exported to Southern California of course increases exponentially as the population of Southern California increases exponentially. And in future years, the fresh water outflow from the delta is going to decrease to a very low level, and these levels will be probably maintained for extended periods of time, during a drought period. And there is considerable concern over what levels of salinity may be reached in the agriculturally-used waters of the delta where many of the crops are non-salt tolerant.

The model has been applied by the Southwest region in addition to San Francisco Bay. It has also been applied to San Diego Bay. In this latter case, the primary interest was in predicting coliform concentrations resulting from the Naval Fleet which was anchored in San Diego Bay. Treating coliforms as a non-conservative constituent with appropriate so-called decay rate. In both cases, the San Francisco and the San Diego Bay, hydraulic verification was pretty much limited to a verification of tidal stage versus time. Comparison of model predictions with actual tide guage records, or with the predictions of the Coast and Geodetic Survey in their tide table predictions. In addition, particularly for the San Francisco Bay system, there were certain data available from state agencies and from the Corps of Engineers on typical channel velocities, flow splits between channels at various stages of the tide, or over a full tidal cycle, so that there was a certain amount of velocity verification, in addition to the tidal stage verification. Now, additionally, in somewhat of a more gross manner, the maximum tidal velocities as predicted by the Coast and Geodetic Survey velocity out of their tidal current tables again were reasonably matched with the model.

Concerning the verification of the quality portion of the model, for San Diego Bay a dye release study was conducted, a continuous point release of dye for, I believe, approximately a two week period. The model then simulated this dispersment from this point release of the dye. In San Francisco Bay, also a dye release study was conducted in the field for verification purposes. The dye release was released somewhere in the vicinity of the western portion of the delta, and again was released I think, in this case, for a period of twenty-one consecutive days, and the results of this dye release was simulated using the mathematical model. In both cases, when the prototype behavior was interpreted in the light of the many anamolies that are generally associated with dye release studies, as I'm sure some of you can attest, favorable agreement was obtained with the rate of dispersment as predicted by the model, as compared with the natural records in the field. In addition, for San Francisco Bay, we simulated at least two different historical periods, one of salinity incursion in the system, and another of salinity repulsion in the system. These verifications, the result of these runs, have been published in the paper by Dr. Orlob, and Shubinski and myself at the National Estuarine Conference at Stanford in 1967.

There was one additional verification, perhaps it can be called verification, that was conducted, wherein the FWPCA used the Corps of Engineers physical hydraulic model to conduct dye release studies using that model. We then simulated the physical model, if you will, with our mathematical model and compared the rates of dispersment of the dye through the system. And surprisingly, or not so surprisingly, we probably got better agreement between these two than we did between either the model and the prototype or the physical model and the prototype. Draw your own conclusions from that. Of course, the model was limited only to the Suisun Bay portion and downstream. The concentration at the head works was held at a constant level. Because in the prototype this did not occur, the concentration actually fluctuates with the tide, we sort of altered the model such that the concentration was held constant to simulate what was actually going on in the Corps Model, and we ran it on something on the order of 30 to 45 tidal cycles.

PRITCHARD: You took the hydraulic model, the physical model, and made its boundary conditions to comply with those in your mathematical model.

FEIGNER: No, the number of different hydraulic conditions that the Corps are capable of running on this model are limited. We therefore simulated with the mathematical model one of the conditions that they could run, as far as hydraulic input go, tides and so forth.

PRITCHARD: Do you have the data from the comparison between your release in San Diego and the San Diego hydraulic model?

FEIGNER: This data has not been published, and I'm not certain what the plans are. I can plead a certain degree of ignorance.

PRITCHARD: It has been reported verbally.

FEIGNER: Yes, right. I think the data would be available from our office. It's at the point where it may be published as part of the appendix to the San Diego Bay Report, or perhaps as part of the documentation of the model if and when that occurs. I have not been involved directly with the Southwest region for possibly a year, and I'm not exactly sure what the status of these two reports are.

PRITCHARD: Simmons gave a report on that to the Tidal Hydraulics Committee.

FEIGNER: Who did?

PRITCHARD: Simmons of Vicksburg, he gave a report on the physical model of San Diego at the last Tidal Hydraulics Committee meeting, and showed some slides comparing the field measurements of dye, the dye buildup and the dropoff after you turned off the dye, with what was observed in the hydraulic model, and they were very close. You haven't seen that?

FEIGNER: No, I haven't.

PRITCHARD: I think it would be very valuable for this report to try to get that information from Simmons.

O'CONNOR: Did that come out of a paper or report?

PRITCHARD: No, that's the problem. So many of these things they do, they do a lot of work with the models, and then their report, say, is to the district, and it's a report telling them what to do, like dig a channel here, you see, and it doesn't contain all of this information. And I've tried to impress upon the Corps that something like this, which has really general information. There are very, very few, in fact I don't know of any such complete comparisons in which a dye release, tracer release, was done in the prototype and simulated in the hydraulic model.

FEIGNER: Because of the limitations of the Corps model, the physical configuration, it was impossible to attempt to simulate the actual field dye release because of the fact that the release was fluctuating. They are in the process now of extending their physical model to include the entire delta, and perhaps after this it will be an additional source of data.

One of the very interesting facts that came out of these various verification runs was the fact that the predictions of quality levels and on the rate of dispersment of a point source or of several point sources was very insensitive to the diffusion coefficient that was used in the model, and it was quite sensitive to the hydraulics of the system, which is probably in contrast with most of the steady-state type models that have been discussed today. And probably the reason for this, of course, is the additional degree of refinement by which the hydraulics of the system are represented in this type of a model.

O'CONNOR: Is that true of both non-conservative as well as conservative substances?

FEIGNER: The number of simulations, if you will, that we conducted with non-conservative constituents was limited. But based on the few that we did do, we simulated, using the present BOD loads, and assuming constant decay rates and reaeration rates through most of the system, we, I would say, satisfactorily reproduced the dissolved oxygen level in the system. It pointed out the problem areas which were primarily around the Southland area, the Sacramento River, those were essentially the two areas where today there are DO problems, and the model did represent these adequately. So the answer would have to be at least a qualified yes, based on our knowledge.

PRITCHARD: What you're saying is that in this model, this analytical model of the delta system, the advective terms dominated?

FEIGNER: Yes.

PRITCHARD: And not the diffusion terms ?

FEIGNER: That was also true in the embayment portions of the system. At least our data indicates this.

PRITCHARD: Was this true in San Diego, too?

FEIGNER: I think the solution there was perhaps slightly more sensitive to the diffusion coefficient used. This also points up one of the advantages of this model in that it is quite easy to do sensitivity type analyses using this model, it's very easy to change the diffusion coefficient to determine how sensitive is the solution to the diffusion coefficient.

PRITCHARD: This was a two-dimensional model?

FEIGNER: Yes.

PRITCHARD: Branched one-dimensional in the delta and two-dimensional in the bay?

FEIGNER: Correct.

RATTRAY: Did this include the nonlinear field acceleration term?

FEIGNER: Excuse me.

RATTRAY: The field acceleration term.

FEIGNER: It's basically the equation of motion which has a momentum term, a frictional loss term,....

RATTRAY: What's in the momentum term?

FEIGNER: $v \, \partial v / \partial x$.

PRITCHARD: Well, you see right away you've taken out of the diffusion term one of the terms that enters the diffusion term, or out of the frictional term one of the terms that enters the firctional. If you go to a two-dimensional net in essence, depending on the degree to which you simulate the two-dimensional velocity field, you substitute for a diffusion process, that is, because the diffusion term in the averaged equation will come out to a considerable extent because you're averaging the velocity field. If you put more detail in the velocity field, your diffusion terms are going to become less important.

HARDER: Perhaps you meant the dispersion coefficient, rather than diffusion coefficient.

FEIGNER: Right, I probably used the two interchangeably without a great deal of thought. I definitely am speaking of the dispersion coefficient.

PRITCHARD: When you say the dispersion coefficient, I'm not sure definitely what you mean there.

FEIGNER: In the light of what's been going on, it's that factor that takes care of everything that we can't describe.

RATTRAY: That's the garbage can term.

PRITCHARD: Yeah, but what's its dimensions?

O'CONNOR: L squared over T.

PRITCHARD: Allright, call it the effective diffusion coefficient.

O'CONNOR: The diffusion, though, has the specific connotation of the turbulent diffusion sense, historically, hydrodynamically, so dispersion is the more general, the broader terminology.

DOBBINS: Dispersion some people choose to represent by Taylor's longitudinal dispersion.

O'CONNOR: Exactly.

PRITCHARD: But in any case, the term comes out of an average of a velocity deviation times a concentration deviation, in both the classical diffusion term and what you call a dispersion term.

O'CONNOR: Right.

RATTRAY: There's an important thing that may not have been caught here. If you're working on a system where you're doing your diffusion steps by half hour averaged velocities, you have already eliminated the primary eddy diffusion that one gets in a real steady-state model, which is the tidal.

PRITCHARD: Right.

RATTRAY: You've shoved that back into the advective term. So it's got to be much less important in this case.

O'CONNOR: That's an important point. Is that what you were saying?

FEIGNER: I believe so, yes.
(Laughter)

PRITCHARD: You have a group of branched channels in an open waterway, which at each point describe the volume of water which goes one way or the other way, and you do this every half hour, your ability to know these, what they would be if you could actually take an average velocity, having these time varying and spatially varying, they just lead to a mixing term possibly because of your ignorance in their true values. As long as there is a time variation and spatial variation in these terms, they are going to contribute to the dispersion.

HARDER: I wish my colleague Dr. Fisher were here. I can only paraphrase some of the conclusions that he's made from studying estuaries, and that is that the value of the dispersion coefficient that is used, even in an analytic solution, the results are indeed insensitive to the value you use, and that may very well explain the very good results that we get from some of our modeling techniques.

FEIGNER: That's probably not a fair statement, if you're using a strictly steady-state mean tidal cycle velocity or flow.

HARDER: This was in an analytic solution he developed for oscillating flows.

FEIGNER: So it would be oscillating flow as opposed to a steady-state type flow, a mean tidal cycle flow which would be unidirectional.

HARDER: This was the mean salinity as averaged from a solution that included an oscillating flow.

THOMANN: I've never been clear on the transition, and I think part of the discussion is revolving about that, that was made in the San Francisco Bay work from the time-dependent advection, with the hour-to-hour fluctuation in tidal stage and current, to the steady-state model. What kind of transition? How did you get, say, the tidal dispersion coefficient?

FEIGNER: You mean when I talked about the steady-state predictions?

THOMANN: Yes.

FEIGNER: The steady-state predictions, when I talk about steady-state predictions, I am again talking about a dynamic steady-state. Given the fresh water inflows, given the specified tidal situation at the seaward boundary, and given all the other inputs and outputs, we run the solution until at each point in the system the maximum tidal concentration repeats itself at each tidal cycle.

THOMANN: Have you tried to compare that approach with going directly to a steady-state model?

FEIGNER: This has been done. But, if you go to a steady-state model, then you have to somehow verify those diffusion coefficients.

THOMANN: But you've got them already, because you did that already with the hydraulics.

FEIGNER: But it's been done, of course, only for a limited number of hydraulic situations. With this extremely complex hydraulic regime, it is extremely doubtful whether one set of diffusion coefficients would be applicable in a slightly different hydraulic situation where most of the inflow, or say, the entire inflow is from the north, where for part of the year, a significant portion of it might be from the south. So there's an entirely different hydraulic regime, and it would appear to me to be very doubtful whether the same dispersion coefficients could be applied in these drastically different hydraulic situations.

O'CONNOR: But you did say that it was relatively insensitive to the dispersion coefficient? More controlled by the advective one.

FEIGNER: But it would not be if we went to a steady-state solution.

THOMANN: But then you could run the hydraulic model once to get the dispersion regime, and then go to a steady-state water quality model.

FEIGNER: But this is a unique hydraulic situation. They would have to be essentially recalculated for every different hydraulic condition. They would only be good, in my opinion, and I think this feeling is shared by Dr. Orlob and most of the people that worked with him on it, would be only valid for a rather narrow range.

THOMANN: I accept that. But suppose I want to run a whole series of water quality analyses given that hydraulic regime?

FEIGNER: This is the additional work that Dr. Orlob has been doing the last two years. The number of junctions that we were talking about just to represent this portion of the system was on the order of eight hundred connected by a thousand or greater number of connecting links. They have a much coarser network which uses longer integration steps, and in which the diffusion coefficient plays a more sensitive role. But the original diffusion coefficient applied were derived, essentially, from the finer grid model, so there has been work along these lines.

RATTRAY: I think that in fact if you go to the steady-state model, the tidal effects on horizontal diffusion result from the errors he finds that are left out of this. Actually the discrepency between this and nature is the thing that causes the horizontal tidal diffusion. The in-phase component of the u as variation. What you're getting here is you're getting out of phase. You get a velocity, and then after the velocity has gone to zero, you measure salinity and that's the tidal effect. But it's the other phase component that gives you horizontal diffusion.

PRITCHARD: Yes, because they're correlated.

HARDER: Following on what Maurice said, it does seem to me that it is significant that when you use a coarser network you have to use a different dispersion coefficient, which indicates to me that something is basically wrong.

FEIGNER: I think primarily because again the time interval involved is longer.

PRITCHARD: The dispersion coefficient is dependent upon the average, and essentially when you're taking a large space, you're averaging over it. When you're taking a smaller spatial interval, you're averaging over less distance. You can show this for instance, well, Reid showed this a long time ago for measurements in the ocean, that the apparent diffusivity, which is the term which has been more commonly used in our field, is dependent on how far apart your stations were located.

HARDER: Well, I'm sure you find an explanation as to why they were different, but I still suggest that if the behavior of our model depends on how we chop up nature, then there is something fundamentally wrong with our models, because of our simplification.

O'CONNOR: No, I disagree, not something wrong.

HARDER: Well, allright, inaccurate.

PRITCHARD: There are two purposes of physics. One is to tell the truth, and the other is to predict. And we can never really tell the truth. For instance, Bohr's model of the atom was quite adequate to predict certain features of gas kinetics for instance, but it's a hell of a model as far as telling the truth.

HARDER: One of the important things we do when we model is to try to get a physical picture, a physical parameter that we can put a number on, and it doesn't seem to me that if that particular parameter depends upon the scale of the model or upon how we chop nature up, that we have done as good a job as we ought to try to do in defining it. All I'm suggesting is that we should look for something that is scale independent.

GALLAGHER: How sensitive was this particular simulation in changes in fresh water inflow? Say with respect to quality.

FEIGNER: In the solution to the salinity incursion problem, it was very sensitive to fresh water outflow from the system. Not so much fresh water inflow here because there were all the factors of exportation and water use within the system. But it probably could be correlated with some other point in the system. But at least one of our major concerns, primarily because this was the number or the location that was normally talked about in the literature, called the Delta Outpost, was that the fresh water outflow or the net flow passed out the delta and into the embayment. And salinity incursion was quite sensitive to this.

GALLAGHER: How about with respect to quality?

FEIGNER: For constituents where there was probably not very steep concentration gradients through the system, of course the system is not very sensitive, the solution is not so sensitive to either flow or dispersion. In other words, things are essentially the same everywhere, so it doesn't really matter.

GALLAGHER: How did that show up in the Delaware, George? Was the Delaware sensitive to increases in the fresh water flow?

PENCE: Not particularly.

FEIGNER: What about salinity.

PENCE: Salinity, yes.

FEIGNER: Yes, I think anything with a fairly steep gradient....

PENCE: You're talking about a different problem, there. The DO in the Delaware is primarily dependent upon the magnitude of the loads and volumes and fresh water inflow does not have very much effect on volumes.

THOMANN: I'd like to also ask the estimated cost per run.

FEIGNER: If I can just postpone that for a second, I am going to get into that.

JAWORSKI: This has raised an interesting point. If your model and the Delaware and Potomac are insensitive to flow, isn't this an indication that we had better go more biological than hydraulic?

WASTLER: The DO in the Potomac is extremely sensitive to flow, and I've got the data to prove it.

JAWORSKI: Well, o.k., the DO at that one point....

WASTLER: There is a point here. It goes back to that scale thing. If you talked about DO variation, and talked about the average. Or you can talk about Norb's eighteen part per million in the morning and in the afternoon, and two parts per million at night, and if you vary the fresh water flow in or out of phase with the tidal cycle, you can produce variations of three, four, five parts per million in the DO. But if you average, say, take the monthly average of flow, or the daily average of flow or the weekly average of flow, then you don't show the variations. But again this is a matter of the scale, and whether it's important or not for a long range prediction, I don't know.

GALLAGHER: So you have to have a very, very strict instantaneous control over flow.

O'CONNOR: No, that would again depend on the estuary, the relative magnitude of the advective versus what we're calling the dispersive term.

PENCE: Let me make a statement and modify something I said. You asked me if DO was particularly flow dependent in the Delaware. In magnitude, no, it is not. In location, yes it is. The Delaware exhibits a more or less classical sag situation. Now, in a low flow situation, the sag is upstream, but if you increase the flow, you move the sag down. So to that extent, yes, it's dependent. If you look at any one point flow will affect that.

FEIGNER: I think that the fact that the diffusion coefficient did play a relatively insensitive role in this particular model has very important considerations when we're dealing with a system such as this San Francisco Bay system, where in future years the hydraulic regime is going to be quite drastically different than it is today. In other words, due to the fact that the solution is dependent primarily on the hydraulics, then the accuracy of future predictions really depends on how accurately the hydraulics of the future regime are represented, as opposed to how sensitive the solution was to the appropriate derivation of the dispersion coefficient, which was probably derived on some hydraulically different condition.

As has been adequately pointed out earlier, there unfortunately is no documentation which exists on this model. There have been various reports on the theoretical aspects, the development of the model, both by Water Resources Engineers and various publications, but nothing that could be construed as documentation *per se* actually exists. There are various FWPCA employees, in addition to myself, who have varying degrees of knowledge on the use of the model, of the model's limitations. Dr. Howard Harris at the Alameda Office probably has as much information as anyone, on the use. Dick Callaway of course is becoming very familiar with it. I think Dan Fitzgerald, who is here today also, is perhaps becoming familiar with the model, also. And also at headquarters in Washington, Lou Gitto, is familiar with the model.

As far as limitations of the model are concerned, I think I want to mention at least two. Of course, right from the start we recognized that the model represents a completely mixed, in the vertical direction, a completely mixed system. If you have an estuary where vertical stratification exists for very extensive areas, and that might be either hydraulic or quality stratification, and if there is concern in that particular area, then it's doubtful whether this model would adequately represent the prototype. However, if the area of stratification is such that there's no particular interest in the quality predictions for that area, then the network for that portion of the model might serve merely as a link to, say, a known source of concentration such as the ocean, or perhaps as a source of a tidal input into the system. For instance, in San Francisco Bay, even though there might be vertical stratification in the area of Golden Gate Bridge, by connecting this system just by perhaps a direct link, it is possible then to specify the tidal conditions at the Golden Gate and have them reflected in the entire system without having to somehow come up with a tidal input at some other point farther inland, which in many cases is more difficult. The second limitation, and this is one that could become quite binding, and now I'll answer Dr. Thomann's question on cost, and that is that it is fairly expensive to solve this model on a digital computer, if you're talking about a large system such as San Francisco Bay. To give some idea of the kinds of running time I'm talking about, we had considerable experience both on San Francisco Bay, which as I mentioned, had on the order of eight hundred nodes connected by a thousand some-odd links, and on San Diego Bay, where the system was perhaps on the order of one hundred and thirty nodes with maybe one hundred and eighty or two hundred links. We also had experience on two different machines, one an IBM 7094, the other a CDC 6600. On the 7094 we were talking about, to reach a steady-state hydraulic condition, a run on the order of one to one-and-a-half hours, which you know can run up a bill in a short time.

THOMANN: One to one and a half hours to do what?

FEIGNER: To come to a steady-state hydraulic condition, utilizing a tide with a 25 hour period. This might involve 50 to 75 hours of actual simulation.

THOMANN: How would I run, then, the problem of, let's say, annual or seasonal fluctuations in inflow, waste load or whatever?

FEIGNER: This would have to be done with separate hydraulic runs. And this is where the cost starts going up, with the number of different hydraulic and quality waste loads you might want to impose on the system. A different hydraulic computer solution has to be obtained for each different hydraulic condition that you wish to look at.

THOMANN: What I'm trying to get at is this, I have a inflow hydrograph of the Sacramento, some condition at the mouth of San Francisco Bay, an inflow hydrograph from the San Joaquin, annual, for a whole year, and I want to take a look at the day-to-day or week-to-week oscillations of, let's say, salinity.

FEIGNER: Generally, we would go to, say, a mean monthly, or perhaps a mean weekly, depending on the degree of frequency.

THOMANN: And run the hydraulic model separately for each one?

FEIGNER: Separately for each one of those with a separate....

THOMANN: And then gear up for that and run for a week or so?

FEIGNER: Right, with a different hydraulic regime. This is contrasted to the CDC 6600 for which that same hydraulic condition could be run on the order of from ten to twenty-five minutes, depending, again, on the period of the tide used and the duration that it takes to come to a dynamic steady-state. Generally, these running times decrease as you build up sort of a backlog of previous hydraulic runs, where the ending conditions from one run could be used as a starting condition for a subsequent run which was hydraulically similar.

HUSTON: You said 10 to 25 minutes, is that 5 minutes per tidal cycle on the average, or 10 minutes per tidal cycle, or what?

FEIGNER: Probably on the order of 7 to 8 minutes per 25 hour tidal cycle. Now, for the San Diego Bay this was decreased very significantly. We had no figures for the 7094, but for the CDC 6600 the hydraulic solution took on the order of, say, less than 3 minutes for an equivalent situation so here the costs are not that significant for a fairly small system.

Regarding the quality solution, which is on top of this yet, again depending on how much time you actually want to simulate, for instance, for a 60 day run where we w [illegible] simulate 60 consecutive days of quality in the system and [illegible] constituents simultaneously, it might take on the order of one hour on the 7094, and on the 6600 on the order of ten to fifteen min. For San Diego Bay, this again would be on the order of one-and-a-half to two minutes. There's a very significant reduction for a small system.

There are also limitations on the size of the network that can be accommodated in various machines. For instance, in the 7094 the system, which included the 800 some-odd junctions and which was cut off here, was pretty much limited by storage limitations in the 7094 to approximately 900 junctions. So if your system is such so that you would want to depict it by more than 800 to 900 junctions, then a 7094 could probably not be used.

The last limitation which I want to address myself to is the problem that seems to be the theme of the meeting so far. That is the concern over the possible enrichment of San Francisco Bay with nutrients such as nitrogen and phosphorus. As I previously mentioned, the model can presently handle the traditional BOD-DO response in an estuary, but has no provision in its present form for the effects on the DO response or the DO resource of such things as production of oxygen by photosynthesis or the depletion by algal respiration. In the San Francisco Bay system, the future projected waste loads, both municipal, industrial, agricultural, and this includes the proposed San Joaquin master drainage which is supposed to dump agricultural waste into the delta system, there are significant increases in nitrogen and phosphorus levels predicted, even with secondary levels of treatment except the immediate agricultural wastes in the delta which probably, because of the wide dispersment of the various waste discharges, are not capable of being treated at least presently. And this fact, coupled with another perhaps very critical factor for San Francisco Bay, and that being that there is a predicted probable decrease in the turbudity levels in the San Francisco Bay system. This is due to increased development upstream, additional impoundments, plus the fact that in future years the major portion of the suspended solids load to the system is going to be exported to southern California leaving much less suspended load to the bay. There is very little data to substantiate this concern; however, there is feeling, even today, that perhaps the inorganic fraction of the turbidity has decreased, but there has been a corresponding increase in the organic turbidity levels due to increased plankton levels. As I say, this has not been substantiated in the field but this is one of the conjectures that has been thought about.

What appears to be most urgently needed in San Francisco Bay is a means of quantifying these various responses in the system in order to predict algal growth rates and crop levels resulting from predicted changes in nutrient levels. Such a modeling effort really requires quantifying all the parameters comprising the so-called eco-system, if you will, which significantly affect the DO levels in the estuary. Some of the factors that are felt may be significant in San Francisco Bay include such things as light and its penetration as affected by organic and inorganic suspended matter including self-shading by algal cells. Secondly, limiting factors including nutrients of several categories. Thirdly, temperature effects on the rates of respiration and photosynthesis. Fourth, grazing by zooplankton. Fifth, vertical movement such as settling of algal cells or diurnal migration of algae reacting to intermittent light exposure. And of course this list could be continued probably ad infinitum.

So in summary, it would thus appear that as far as the Southwest Region goes, the major concern as apparently it is in many other regions, is the development of an ecological model which would include these biological factors in the DO predictive system. I would also like to comment on one of Dr. Thomann's earlier points, in that he commented that going to a very short time interval was perhaps not necessary in that many of these inputs were of a stochastic nature, and therefore, it's probably not too valid to use a deterministic-type of model. But as I mentioned before, this model is very well suited to do sensitivety-type analysis, and I think it is very possible to, at least, get some good feel for how sensitive the solutions are to these various parameters, whether they be stochastic or deterministic.

SHULL: Not picking on you in particular, but several people throughout the day have been talking about the success that their models have had, and have used terms like "adequately represented", "reasonable agreement", and "verification", things like that. Would you have some sort of statistical methods for quantifying what we mean by this, and I wonder how many of the others have gone into this? For instance, what do you consider "adequately represented"?

FEIGNER: Usually if you can find enough data at all, that you feel fairly represents the system, so that you can even make a comparison, usually you're very fortunate. The data is so limited that it is doubtful whether a statistical analysis is that valid. The results of a statistical analysis may be highly questionable. It's certainly a good point.

* * *

ESPEY: Ken, that was a very good talk. Thank you. Now we have the Northwest Region. John represents the Northwest Region.

VLASTELICIA: My last name is Vlastelicia and I feel with a name like that I should apologize at least for not having a good Polack joke to tell. The name is Yugoslav, and where I come from you just don't joke about Yugoslavs.

(Laughter)

I represent the Northwest Region. We have three coastal states, Oregon, Washington, and Alaska. I think we have probably a greater percentage of estuaries in our region than any region does, and most of those are in Alaska. I regret somewhat that I have nothing really to contribute to the state-of-the-art here today, but that doesn't mean that I won't take some time and talk. It never has meant that I wouldn't talk before. Mr. Callaway already has covered pretty much our major modeling effort in the Northwest Region right now, as far as estuaries are concerned. We have had some limited application to other estuarine problems, and maybe I'll get to say a word or two about those. We are looking forward to applying models to estuaries for a number of different problems. We have need for application, and I'd like to address myself to that point for just a moment. I reviewed the outline for the state-of-the-art report, perhaps not thoroughly enough, and I've listened to the presentations here today, and I heard about all kinds of models, electrical-analog, hybrid models, stochastic models and digital numerical, the only model I think I haven't heard about today is a Christian Dior fashion model. And in light of the interesting evening that's planned this evening, I understand, I think I'll withhold my comments on that. But I'm overwhelmed by the variety of models that we have available to us and the kinds of applications that we might make of these, and I'm sure that this state-of-the-art report is going to be a real contribution to what the regions need. But I see a possibility for at least a note of caution in putting together this state-of-the-art report. And that is that I think you could adequately put together a total state-of-the-art report on analog modeling, and still not answer the questions of the regions. I can see, I think reasonably, we have a state-of-the-art in all of the technical literature around, and a great deal of it starts off with "assumming...," this is usually the first sentence right there, and then it gives equations such as this, and then it comes down a little farther and says "therefore...," or generally it'll say "obviously..."

(Laughter)

And the title might say "Numerical Solution to an Estuarine Problem." But really, unless we're really familiar with the literature and application of this sort of thing, I can see a completely adequate representation of analog modeling or any types of modeling that would not answer the questions of the regions. So I would add my plea to those of the other regional representatives that we've heard here today, that if we could orient this state-of-the-art report to problems if possible, not so much to methods of solution, but to how to solve the problems. I don't know how feasible that is, to suit the R & D purposes, but I think it would need to be oriented that way to suit the Nation Estuarine Pollution Study purposes and the regional effort. That's enough philosophy for today.

In the Northwest Region I think we have about every kind of estuary that you can think of. We have at least all three of the classic classifications of estuaries of Dr. Pritchard's. We have the well-mixed estuary, the partially-mixed, the two-layer system. We have some estuaries that go through these phases at least once every tidal cycle. We have estuaries that aren't classified, such as in Southeast Alaska, where the major fresh water source to the estuary comes from the seaward end, and this is made possible by the formation of a great layer of fresh water up there during the summer when the glaciers and the snows and icebergs melt, and there are some estuaries like Gastineau Channel, where the driving force for circulation is fresh water entering from the seaward end. This makes for very interesting problems for solution. I don't know how much the state-of-the-art might contribute there. I don't really believe that that particular situation is in need of solution at this time. But we do have a variety of estuaries and estuarine problems in the Northwest Region.

I think there's probably certainly no one model that's going to model these things for us. The thing that makes our estuaries different is perhaps the dynamics. I think the average tidal cycle in and around Puget Sound is something like seven or eight feet. And reasonably most of our coastal plain estuaries having a good fresh water inflow are swept completely clean of fresh water over a tidal cycle. Consequently, we are not generally interested in problems of eutrophication, in problems of low dissolved oxygen. There are some exceptions to this, but these are not the kind of problems. We're in an enviable position I think, of not having to restore our estuaries, but we're in a position now of thinking about models to use them to protect and manage our estuaries. I think maybe there's a little bit of difference in how you look at a model and what you use it for from that standpoint.

Listening today, I see generally there are two kinds of models for two different purposes. One is a local model, an outfall model perhaps of a waste source. We have need for such a model in the Northwest Region. We would like to be able to predict from an outfall source how the waste disperses into the stream down to some point where it's reasonable to assume that it's well-mixed into this homogeneous estuary section. We would use this, I think first of all, for analyzing thermal effluents. This is our big problem now. We have need for looking at this aspect of it. Pulp and paper mill wastes is another one, large sources. I think we would look to using such a model to determine the impact of dredging in an estuary, at least the outflow of a dredge, or barge waste disposal along the coast. We have need for this.

The other kind of model, I think besides the waste source model, is the general model that we've been talking about. And this one makes some broad assumptions about an estuarine zone, and models it generally for the purpose of management, I think. That is, it takes a management outlook to it. I think we run into the question here of defining an estuary. This morning we talked about the salt water zone being the estuary, and the freshwater tidal portion as being the tidal portion of a river. I don't know how you would define an estuary, and how big you should make this model, but it should be responsive to the upland flow, I would hope. I hope that this can be taken into consideration. At a public estuary in Seattle about a year ago, Al Wastler was there and he may remember, we ran into this problem of how do you define an estuary. And a verbose gentleman, a good biologist and a great conservationist, but a foe of federal intervention, was there, and he said that if that past experience was a guide, that he felt certain that the federal government would have no trouble defining an estuary as that body of water extending from the snow fields to the sea. I disagreed with him at the time, but in talking about an estuarine management model I'm not so sure that I do disagree, in that I think when we talk about what's happening in an estuary we're going to have to at least make it compatible with the input from upstream. I would hope that the state-of-the-art will be responsive to that need.

It would be nice if we could blend these two models. We could use this in the Northwest region; if we could take an outfall model and work it into a general estuary model to the point where we have only one model, and I would ask the state-of-the-art to determine whether or not this is possible. We would use a general management model in the Northwest Region, I think first of all for thermal management like the one that Dick is working on. This is our big problem. We would hope to extend it to other quality parameters. We would like, I think Tom Gallagher mentioned, to use a model like this to determine what should our data gathering needs be in an estuary. Can you give us a model and set of, not instructions, but guidelines and discussion about how to use a model in an estuary zone, to guide our surveillance program? We spend some $300,000 in our region, the two states Oregon and Washington, in the FWPCA for monitoring. I don't think that we're doing a good job of this.

Our signal effort in Northwest Region for estuary modeling is the one that's been talked about here all day. It's signal not only because it's applying temperature, which is a very hard parameter I think to incorporate in a model like this, but it's signal also because it's being worked into a total basin model. Some of you will be happy to know that Mr. Callaway does recognize the one-dimensional model, and we have had an application of this. I thought that it was a fairly nice application in Grays Harbor in Washington. Dick used Stommel's approach, I think without the parsnips as I recall,

(Laughter)

to model the dissolved oxygen regime of grays Harbor. Grays Harbor has an annual dissolved oxygen deficit problem from pulp mill inflow at the upper end and from advective upwelled low dissolved oxygen water from the coast. Dick used his model to analyze this and actually, I thought, got fairly good verification, and here the word is open to interpretation. He used it to project the impact of upriver flow regulation.

We've had some small experience in our region with small-scale hydraulic models, and I would like to know for our region what is the state-of-the-art here. How useful is a very small-scale model, a highly distorted model for pollution analysis? A study was run on the Umpqua River with a distortion of 30 to 1, vertical to horizontal. The model was build on a 1:3,000 scale horizontal and a 1:100 scale vertically. With this distortion of 30 to 1, and a salinity ratio of 1 to 1 in the model, they were able to reasonably verify the tide propagation, the salinity patterns, and the general circulation in this estuary. And it does seem to have some useful purpose. It does not reproduce point velocities certainly and the mixing or the dispersion of waste.

Another model that we've had some small association with, and Dr. Rattray I'm sure could go on about this model for a long time because I believe that he was in charge of the construction of it, and that's a model of Puget Sound, a very small scale model, built to a horizontal of 1:40,000 and a vertical of 1: 1,152, for a distortion of about 35 to 1. This model has also been verified reasonably for salinity, stratification, seasonal sycles, and general circulation and tide propagation. We had an opportunity to use this to study a very small portion of an area we were interested in. We determined net currents in Possession Sound, an area about as big as your hand, and reasonably about 8 miles across in the prototype. And from the information we got on net circulation, we were able to, I thought, support what we thought were unlikely waste disposal patterns, that we observed in the estuary. So the models of small scale have some use certainly, and thinking about southeast Alaska and the complex system in Puget Sound, the kind that we are not likely to model with a mathematical model can you tell us how much we can do with the small scale hydraulic model?

That is the extent of our estuarine modeling experience in the Northwest region. Probably as significant as the modeling that we did do is the modeling that we didn't do, but have the answers for. I think from what I can hear today, if we have an estuary that flows upstream and downstream, we have a pretty good problem, we have one that we can get our hands on and do something with. But, as Dr. Jaworski mentioned, give us something to work in an embayment with. Here's a tough problem, it is five miles across or four miles across and ten miles long, and subject to the most gosh-awful set of tides, winds and fresh water flow that you can think of. But it is a rather small problem and you can't go in there and build a hydraulic model of it because it doesn't justify the money, and you wonder what to do with it. We didn't know what to do with it; it was part of an enforcement action. There was a waste source, a pulp mill, up in this corner of this embayment. Through a set of surveys, made both by the University of Washington and FWPCA, we took data at monthly intervals over some long period of time, maybe a year or a year-and-a-half, over about 20 stations in here. Taking the average of the data and plotting it out here in terms of a plume at the surface, you get this sort of an average picture of a plume. What we thought was a fairly random, tough situation to look at looks pretty well-behaved. We had seventeen current meters out here recording for a month, and were not able to come up with anything concrete in the way the currents worked out here, but the long term pattern of waste did look pretty good. Looks even better if you'll take along this plume core and plot the concentration gradient. When you do that, you get a drop off right here, right in the harbor area, where it goes off, and then it just goes like that, and the points that make up this line are all on the line. It's not an average fithere, all those points are on that line. And what looked to be a very hard problem to look at, one that we can't write the equations for, the model for, the solution is a fairly straightforward solution. And this is the solution for most of the models that we've been talking about today, the one-dimensional-type estuary model, and so there must be some way that we can look at an embayment and do a better job of it, when it acts like this. Dick first noticed this in the Everett area, where waste was discharged at depth and kept at depth, and even at depth, for several miles out, it plotted in the same manner here. This thing acts even better from a statistical point of view. If you take the data at any one of these stations, and you write this data in terms of decreasing water at that station, and plot it on probability paper you get about as straight a line as you get up here, very normally distributed right there. And the probability of a certain value occuring here seems to be pretty well-defined. In some areas here where we have a fresh water plume sometimes come across in the river right here, you got a break in the curve. I think it went like this. You have a family of two curves. This one was associated with salinity greater than 10 parts per thousand, this line associated with the salinity less than 10 parts per thousand. It had to do with the specific gravity of the effluent going under fresh water. I don't know what this means, and I don't know whether anything can be done to help look at estuaries like this. But they don't all act to the point that you can't do something with them. What kind of sampling program might we have in an estuary like this that would optimize, that we could make best use of our efforts, and still come up with something that would perhaps relate this thing that's happening up there, and give us some sort of feeling for this estuary? We could use an interpretation like this in our surveillance program. How many stations should we be taking out here? How often should we be taking them?

QUESTION: What was the name of that again?

VLASTELICIA: This is Bellingham Bay. It's generally called in Puget Sound, but it's not Puget Sound proper; it's adjacent water, but it's inland water in the State of Washington.

QUESTION: About how deep is it?

VLASTELICIA: It varies from about 30 feet deep in this area to, say, 150 out here. It has a net outflow, at the surface, fresh water layer and we suppose it has inflow at the bottom.

But looking at that, I think we decided we couldn't sit down and write the equations for it, and I'm not so sure we can at this time, but we can write the answer for it, and it seems like such a good answer that someone could write the equation for it.

(Laughter)

O'CONNOR: I think the equation for that has been written, and it is of the form of that solution right there.

VLASTELICIA: I think that the University of Washington in their second look at the data from Bellingham Bay did publish, I think on radial coordinates as I recall, or maybe it was just in two dimensions, I don't know.

O'CONNOR: I applied that type of model to some of the coliform distributions in Michigan Bay.

VLASTELICIA: But could you do this before you looked at this situation, and predict this kind of thing? That is what we would like to see presented in the state-of-the-art on a problem-oriented type discussion.

O'CONNOR: I appreciate that.

BAUMGARTNER: You could, but you still need some of the data to get K, wouldn't you, Don?

O'CONNOR: Yes. We got the K from some of the work of the oceanographers on the scale phenonema, and some of the dispersion studies the Public Health Service carried on in Lake Michigan. The spread of those flags and drogues, you know.

VLASTELICIA: With that, I will conclude. Somebody told me at lunch-time that the definition of a tidal bore was somebody at this conference talking after lunch.

(Laughter)

* * *

ESPEY: Well, we would like to change gears a little bit now and get into an actual discussion of the contents. We have prepared some outlines which we would like to pass around the table, and also open up the discussion with a little five or ten minute talk by each of the consultants. Then get into the discussion of the chapters. George, would you want to pass those out and relate Dr. Harleman's thoughts to the group?

WARD: Dr. Harleman, as you all know, is in England and was unable to make this conference. However, he did furnish us with a brief, rather skeletal, but nevertheless detailed enough for our purposes, outline of the material he would include in the two sections that he will write provided, of course, that the report has the same outline form after today's session as it did before. So, I will pass these out as far as they will go, and I think it's all self-explanatory. With the price of transatlantic telephone calls these days, we couldn't go into a lot of detail on this. I think what Dr. Harleman has written here exemplifies some of the thoughts the participants have expressed during this meeting, that maybe a method-by-method approach is not the way to go at this. Within each individual chapter, you are more or less constrained to redevelop all of the equations you intend to use to lead up to the particular technique that you are discussing.

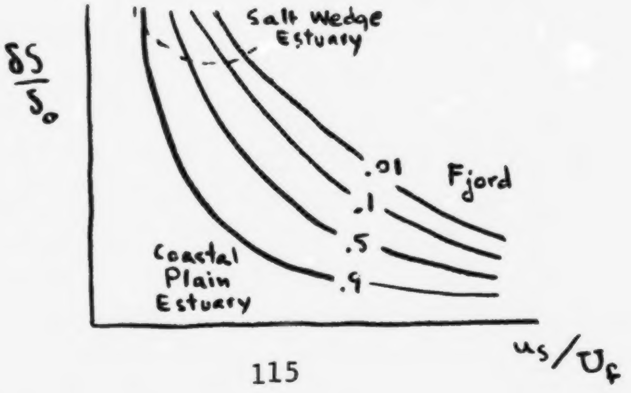
ESPEY: Do you plan to discuss this in any detail?

WARD: No, it's self-explanatory.

ESPEY: O. K. Well, why don't we proceed around the table. Dr. Rattray, possibly you have some remarks that you would like to make at this time.

RATTRAY: Last Friday I was sitting here in Bellingham Bay watching a sailing brigada. All the boats that went this way got there faster than the ones that went this way, every time. So either there is a difference in wind or there is a difference in current across here, which is there on the average. And when you send twelve sail boats out in different directions and they're very equal speeds, as they are, you have a very sensitive measure. I am sure the currents are quite variable but I think that, averaged properly, something would in fact show up, and of course this kind of a distribution that you get averages out the currents' effect reasonably well. And in all of these tidal situations, tidal currents for example, are very much more variable than are tidal elevations, which are an integrated effect of the tidal currents. This has to be remembered.

My concern is in the applicability of modeling estuarine circulations so that the various parameters which will have to be put into the kinds of equations that are integrated can be understood and predicted, hopefully eventually, before you even go make any measurement, that is, any measurements of velocity, salinity, like that, from the gross external parameters, such as river runoff, tidal range at the mouth, and geometry of the system, as you really have separated things. This means that a dispersion coefficient which changes with scale and changes with river runoff isn't a very adequate description. It's not something you can talk about until you have tried it, and you've twiddled knobs and you've found something that checks out a salinity distribution. The trouble being that the same setting of the knobs may not check out some other distribution, most likely won't unless you have a exceedingly simple system. Now, there's been quite a bit of discussion about one-dimensional models, and Don Pritchard and I had a little confusion of what we were talking about on what a dispersion coefficient is in these one-dimensional models. It is possible to construct a diagram on which you can classify estuaries and, with the two dimensional vertical circulations included, draw in at least an approximate fashion, the relative upstream salt flux due to longitudinal diffusion and the relative part of the upstream salt flux due to the advective circulation due to the density salinity distribution. And just to refresh some memories, or maybe some that aren't familiar, intuitively you classify estuaries on a diagram which says circulation, this would be the surface velocity divided by the mean fresh water flow through a cross section, and here is a difference in salinity top to bottom divided by the mean cross-sectional salinity; and grids of constant value of the diffusive fraction go on something like that.



The diffusive fraction being .01, .1, and .5 and way down here .9, something like that. So that there is quite a range down in this region. The diffusive fraction is more than 90% of the total upstream salt flux, and in that region the one-dimensional model is going to be a pretty adequate representation of what is going on. And the apparent dispersion is not going to be very markedly affected by changes in river runoff or changes in other parameters, becuase it is a real tidally-averaged longitudinal diffusion term that is primarily causing the upstream salt flux. But that same kind of a model won't apply when you go up here, fjords are up here, and of course in our area, we are sort of interested about what happens in fjords. There most of the salt balance is advective, and that is what really separates a fjord from down here where coastal plain estuaries are, which in fact go up to about here. Up here are salt wedge estuaries which are different again, in the sense that tidal mixing is not sufficient to break down a very strong density gradient at an interface.

CALLAWAY: Can I ask one question? How and where and how often do you measure U_f?

RATTRAY: This would be an average over a tidal cycle.

CALLAWAY: In the center?

RATTRAY: Or an average over a cross section.

CALLAWAY: How do you measure it, or do you really want to measure it?

RATTRAY: Yeah, this is a tough one to actually measure. It's certainly a variable for which you have to get a number of measurements to get a good average on. But it still shows you the way things go when you do it. It is possible again in this region to superimpose grids which intersect points that are dependent upon the mean tidal velocity at the section, the mean river runoff velocity at the section, the depth, and the density difference at the section. This gives you two sets of lines which intersect, from an empirical fit on too few data, as everybody's pointed out, but does give some estimate at least from these external parameters where on this diagram a particular estuary might fall, and some guidance then, at least on the range of parameters one might expect to find in that system, and whether in fact a one-dimensional model would be a reasonable sort of approach or whether it wouldn't. Now, that's for a one-dimensional model, even when we use it only for the salinity distribution, because certainly something, in our case, for example, where you have algae blooms that are pretty much totally in the upper layer, they're going to get moved horizontally much differently than the salt which extends through the total depth, so that a horizontal diffusion process may be fine for the mean salinity, may not do you any good at all for an algae bloom. So that again even here you have to worry about different scales, different distributions being moved around and dispersed differently, depending upon their scales.

So, I guess, what I would make a plea for is that the state-of-the-art leaves plenty to be desired.

(Laughter)

JAWORSKI: Is that written up any place?

RATTRAY: Yes. Unfortunately I picked up the wrong briefcase when I left as I rushed. I must have been really rushed, I left quite a few things, like a razor. Limnology and Oceanography was the journal, but I can't give you the issue, 66 or 67, I guess, but I'm not sure. Limnology and Oceanography was the journal. It's been verified to the extent that independent investigators from other places have been able to identify their measurements and characteristics, and locate their estuaries on it. And this recently happened again in a recent issue of Limnology and Oceanography, where an Indian estuary again sort of fell in there independently. Becuase there are some good data, thanks to some people here, in the coastal plain region, we have some measure of empirical fit with external parameters. We don't here, and we don't here. There just really aren't the data available, either on salt wedges or in fjords to locate oneself on there by the external parameters of river runoff, depth, and so forth, as one can at least approximately do with the coastal plain estuaries.

PRITCHARD: I think it's desirable that anyone looking at this paper to look up the previous paper in the journal. Your research on the solutions. They're related.

RATTRAY: They're related, certainly. And one of the questions here is when does a salt wedge estuary break down and become either a coastal plain or a fjord.

O'CONNOR: The basis of this is some of your original work on similarity solutions?

RATTRAY: Yes. This is an extension of that, in which also field data on these three external parameters gives you two non-dimensional quantities from which you can put a grid on here. But it does show definite gaps in the knowledge of regions you can do that. And it does show some of the difficulties one might expect from simplifications in the model, where they are good what they're good for, and maybe whether they're not good. This is all based on a steady-state averaged over tidal cycles. Certainly, this would give a mean distribution about which I would think some of the tidal models would allow the salinity fluctuation.

to be moved back and forth pretty much more accurately with a one-dimensional model in that case. The difficulty being, and I think we see it in Puget Sound, where you have irregular bathymetry, you don't have a bulk movement of the tide back and forth, but essentially the bathymetry will couple the surface tide to the density distribution, and you get an internal oscillations of tidal period of the density surfaces with a bathymetric coupling to the surface tides. So that where you have the sills and deep basins and sills, you will get oscillations in the stratification. And therefore current velocities, tidal velocities, are not in phase throughout the whole vertical. So here again are another set of problems which have to be investigated somewhat independently from the simple one-dimensional model.

I just think that we will all be in business for quite a bit longer with good problems to solve.

(Laughter)

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ESPEY: Dr. Dobbins, would you care to make a few remarks?

DOBBINS: What I've got out of this up to now is that if this report is to really be effective and make a contribution, it should be presented in as clear a fashion as is possible. And it should be a report that is prepared for those who are going to use it, and not simply a bunch of papers written from one expert to another. I remember reading a discussion in the Sanitary Engineering Division Journal of ASCE a couple of years ago by a professor, I guess I shouldn't name him since that tape's running there.

(Laughter)

He started out with the statement, "The purpose of a paper is to clarify the issue and not to confuse the reader," and thereupon he proceeded to tear the paper apart. I'm really conscious of this, I think, especially right now, because I have cast off the academic robes and now am spending full time as a practicing engineer, and I can't help but at times to think how much easier it was to be a professor. I could write a paper and conclude it with a long list of sweeping conclusions and recommendations, and I could sleep at night knowing I didn't have to take any action on any of them.

(Laughter)

Now I must reach agonizing decisions with little or nothing to go on most of the time, and of course, I have to deal in areas that I haven't had time to dig into in the past.

We all know that an estuary is an exceedingly complex thing, physically, chemically, biologically. I suppose if we try to come up with an explanation of what the ultimate objective of this thing would be, we would finally come up with some method by which we could write an equation that c is a function of x, y, z, t, that we could define the concentration of anything we're concerned with as a position of space at any time, and where time means real time, after some $t = 0$ in which we had some definable initial distribution. Of course we can't ever hope to accomplish this so we have to start simplifying things. And we start out by classifying estuaries so that you can take a different approach to each one, and then when we get one that's long and narrow, we'll cast aside the y and z and make c a function of x and t, and then we even further simplify it by defining t as the numbers of tidal cycles. I'm not saying that we shouldn't do this, this is the only way we can approach it. However, I think that in the presentation of these things, we should make every attempt to make it clear to those who use it [illegible] what the assumptions are, and what their limitations are, and so on.

The first simple models where c was a function of x and t, I suppose were the Streeter-Phelps equations. Then the term diffusion--if that's the right word--coefficient was used, and I think we all realize now that it's a garbage coefficient. It contains everything that cannot be put into the equations, but actually does exist in the real system. Also, I think we have to admit to ourselves that verification in many cases is not so good. What we do really verify in most cases is simply that the response has a shape which can be defined by a certain function, provided, however, that we make a garbage coefficient a certain magnitude. And as we keep adding terms to these equations, taking in more and more of the sources and sinks, we have to keep readjusting the value of the garbage coefficient. Ultimately, of course, we should try to develop models in which t can now be timed by the clock and not simply the numbers of tidal cycles and so on. Certainly advances in this direction will be very slow, so the steady-state models will have to be around for a long time. I guess we can be consoled by the fact that the variations..., that is if we can show, based on the steady-state concept, we can draw a band around these curves in which we can expect most of the temporal variations. Of course, we have no process like a true steady-state. In the first place, we're dealing with two fundamental frequencies here, the tidal period, 12½ hours, and even that's not constant, and the inputs which have a 24 hour period. We have all of these diurnal variables, whereas the hydrodynamic basic period is the tidal cycle. And we consider this along with the fact that all of our inputs are of such an erratic nature that obviously no such thing as a true steady-state ever could exist. Again I'm not saying that this isn't useful. This is the only thing we can do. But I think that when we present these things in this report, we should try to define our terms, clarify our terms, and put all the assumptions that we make down. So that, for instance, when some local engineer tries to use this, he realizes this K really is a garbage coefficient, and it isn't something that can be too clearly defined.

I know some of the work I've been doing recently in the comprehensive studies within New York State for long range plans for pollution abatement, that a tremendous amount of work is being done by many engineers who simply cannot understand this stuff which is in the literature today. And, therefore, they fall back on the Streeter-Phelps equations, or something like this to work their problems.

I think that's the message I got out of this morning's presentation. The purpose of this report is that it can be used, can be understood and used, not necessarily by every practicing engineer, but at least by those who are charged with carrying out this type of work, and by many other responsible engineers who use it in their individual private jobs.

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ESPEY: Thank you, sir. Bob, do you have anything to say?

THOMANN: Just one or two points, and then I'll pass it on to Don. It seems to me that again we've heard several times this morning and this afternoon the notion that there are a number of models around, and in fact there are really relatively few, both from the hydrodynamic point of view and from the water quality modeling point of view. Again, there are different methods of solution, but we really only have, in the sense of a water quality model, one model that we can write down without too much difficulty, and that's a multi-dimensional partial differential equation with advection and dispersion, and that is about it. Now whether the solution of that equation feeds forward into another equation, such as in the BOD and DO, that's conceptually a relatively easy extension to make. In a sense we really have a set of models that from a steady-state point of view I think we've come quite a long ways over the last half a dozen, or eight or nine years. One-dimensional steady-state water quality models I think we've discussed quite extensively. Multi-dimensional steady-state water quality models are computationally now available and are beginning to be applied on a much more rapid basis. Coupled water quality models on a multi-dimensional feedback notion we are beginning to apply now in some of the work up at the college; this includes some work on nitrogen feedback, algal storage of nitrogen, and so on. All these models are linear first-order kinetic models, and I think those kinds of assumptions, and here I fully subscribe to Dr. Dobbins' remarks on clearly stating some of our assumptions. From a nonsteady-state point of view, I think most of you are aware that a one-dimensional water quality model is available in various forms.

But the purpose of these models, I'd like to see very clearly stated in the report. And that is, by and large, if we're writing this report for practitioners, for those out in the field, for those who face the day-to-day operational problems, we'll have to keep in mind, quite extensively I think throughout the development of the entire report, the notion that the models we are particularly interested in should have a sort of pragmatic air about them, that they're there to solve specific problems. And that means that in any number of problem contexts we may very well be able to take relatively large cuts in space, relatively large cuts in time, recognizing that we're not answering a scientific question, we're answering an engineering question. By and large, the kinds of questions we face require at least some sort of analytical framework that we can interrogate, that we can plug into to provide at least a first-cut answer to some of our problems. This of course raises the whole verification question, and I think we'll have to discuss verification specifically in the report. It's curve fitting to an extent, but of course the whole objective of verification is to sort of provide one with a little more confidence that the equation that he's written down and the coefficients he's used make some sense. Given that, he has at least some degree of confidence in continuing, and answering some questions posed by the administration.

So my plea is to recognize that at the present state-of-the-art water quality models are really one, and that's a multidimensional partial differential equation which may or may not feed forward into other variables. To deal with these other problems concerned with nutrient transport, eutrophication, and so on, requires a series of models that are quite different from what we've been talking about today.

* * *

O'CONNOR: I'm afraid Bob and I have been working too long together, at least a year. I agree with everything he said. That's a dangerous sign. But I made the plea this morning that we should perhaps pay more attention to some of the kinetic aspects of the water quality problems we face. Perhaps if I could take five minutes, I'd like to briefly review the algae dynamic model that we've been recently working on, simply to testify very quickly as an example of what I think we have to get into very, very soon. And I will limit myself to five minutes, Bill,well, maybe seven.

(Laughter)

Given this problem. I'm trying to point out two things by this small example. One is that I think that for the past five or ten years we've made some very significant progress, still incomplete, but progress along the flux end of our equations, the dispersion and advection terms. And we're still far away from some of the significant kinetic terms, biologically and chemically, what I'd just like to show for a little bit, and ask your advice as to whether we ought to go this far in this report. But I would like to point out to you something we've just completed, and, actually, it was with respect to the problem of the Delta, although I hope it will have more general applications, this algal dynamic thing. In this case, we took a very crude spatial scale, and were looking at the seasonal variation of algae. So we just took a completely mixed system. What we did then was simply bring in the fresh water flow from the San Joaquin and the Sacramento and let that go out. We limited it to about a tidal excursion, and looked at what would happen over the year as to growth of algae. And so you can roughly appreciate our time scale, and get some feel for averaging data over a period of a week. Given that context we were trying to answer, say, what's going to happen when you return this, or specifically the question that was asked us, very correct engineering questions, Bill, is when they put this purple canal in, how will they set the gate structures here to admit flow to keep things flushed. That would mean what Q do you want to put into this thing, given this projection for the future. And we just did this, the San Joaquin and Sacramento flow. We're going to look at the growth of algae in this bucket, so we write very simply then

$$\frac{dP}{dt} = \frac{QP_o}{V} - \frac{QP}{V} +$$

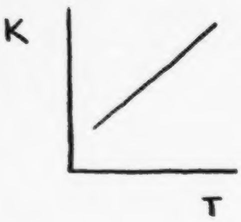
where P is the concentration of algae. What we bring in is some background concentration in the incoming flow, QP_o divided by V the volume of the bucket, and then what we bring out, it's a completely mixed system, so what we bring out of course is the concentration within the thing. Now we just put down the sources and sinks. The algae growth, of course we go right back to our first order kinetics, the first coefficient we need, they die a natural death in accordance with their concentration, and then they're grazed upon, and the grazing of course is another coefficient. And it will be, we reason, not we, but men like Riley and many others, have reasoned previously, that it will be a function not only of the concentration of the algae, but also of the concentration of zooplankton.

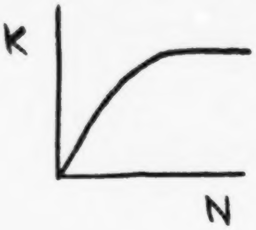
$$\frac{dP}{dt} = \frac{QP_o}{V} - \frac{QP}{V} + K_1P - K_2P - K_3PZ$$

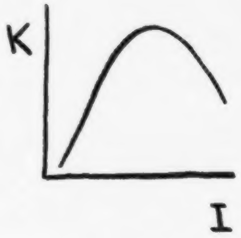
$$\frac{dZ}{dt} =$$

And there you see one nonlinear term, so we introduce Z and we can start the same thing with Z. And of course you go through very similar kinds of reasonings, and you can even put in their grazing by the herbivores and carnivores and so on..., you know. But I think for the problem we have here we stop with this level, and we just have a growth and death term. And now we start to look at things like this, how do they grow, and you introduce all the things, the questions that were asked us before. We know what factors affect their growth. Well, temperature affects it. Take this K, our first order K, this growth K is a function of temperature, and we have some pretty good empirical data on this. It is a function of the nutrients. It's a limiting growth, and we can formulate that readily. And it's a function of the light intensity. There it goes up to a limit and then drops off, and we can formulate this. We know the extinction coefficients, and we can average this. It's very crude. We're averaging this over the depth as well as the volume. So we incorporate all of these things in here, and you can see it's fairly nonlinear, because this K_1 becomes a function of temperature, nutrients, and sunlight, and that means you have to model the nutrients, and then you go to the same routine with the nutrients.

And just bring them out and that relates to the growth. So they're all coupled, and you can keep going like this. And you finally just have to put in the various factors that we want to take into account, the Q, the temperature, and the sunlight.



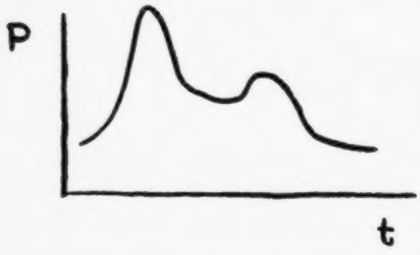


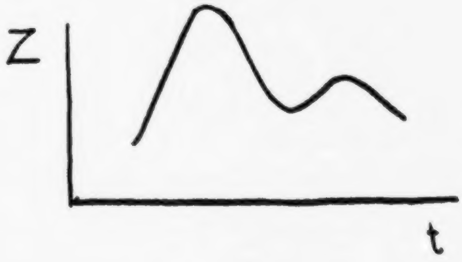


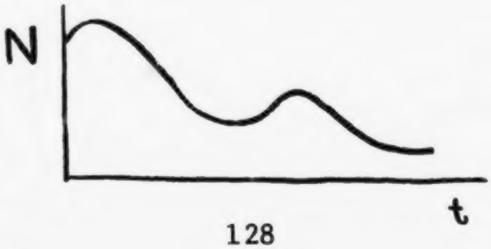
All these things go in, and we've gotten very fairly good verification. What do we mean by that?

(Laughter)

To pick a pattern of growth, in this we have a few measurements of the pattern, there's a spring bloom, drop off in the summer, and then another slight fall bloom. And the zooplankton traffic, there's a little bump here, and then the nutrients come in high and then drop off, and then build up again. And we generally got these shapes out of this sort of thing.







Highly empirical in the sense of being completely mixed, but very functional, with the idea that possibly you can make some good predictions. We had to have some clues as to how the system was working. This is why our historical data is so important. You have to know what's roughly happening, and then we can model it. We're just starting now. Once we do this, and say we make some reasonable projection, how reasonable is that going to be? Well, we'll find out when we change something and go back. Then we revise the model and so on. I think we're just finally making some sense of this, and the verification procedure is going to be finally tested on what you as an engineer and I as a professor, do with it.

(Laughter)

Between the two of us, this sequence, the usual development of science. But we just started. Our BOD models, the Streeter-Phelps, I think so far as one-dimensional BOD and DO, I think we're in pretty good shape. But really we have never verified it in the sense that of saying put in this treatment and what's going to happen. And what we're doing here, we should do the same thing with the BOD, the carbonaceous, the nitrogenous, and you see that ties in with the nitrogen. So really, the total thing is going to be one... But we're not going to go that far right now, but we know that some day we'll probably have to.

I want to conclude by saying, then, that I do feel that probably now that we've made pretty good advances on what I just erased, we should go back and look at the engineers..., not that I'm telling the oceanographer not to continue with what he's doing, nor the hydrolician, but I think when you see from this too why we feel like Orlob's model is probably too great a refinement for what you're finally going to get out of this thing. You know what we have, we have a nice simple T, the detention time in a settling basin, you know, the good old two hour detention time. I feel the same way when I use this by contrast to what we have available hydraulically. But at this stage, I think it is probably all that is necessary. My first point was just to bear out, to get some balance on some of the kinetics. As a general question that I would pose to the group, do we want to go this far?

COMMENT: Yes, definitely.

THOMANN: Could I make two comments in addition to that? Well, I think this highlights what I meant by a model versus a computational technique. There's the model, and actually it was a series of four coupled nonlinear equations. Now the problem is how do you go about solving that with arbitrary coefficients. In our case, we used the continuous system modeling program, which is analog components that we just write on a digital computer. One could equally model that system totally on an analog computer. But that's really irrelevent to the conceptualization of the model.

And the second is the addition of spatial detail. I think here the addition of spatial detail, conceptually, is simply the addition of the flux and the dispersive term, with an order of magnitude increase in computation. But that's not really our concern at this stage, that is, adding on spatial detail. It is reaction coefficients.

O'CONNOR: The spatial detail, by the way, that we are adding on, we're simply putting in exchange similar to what Ketchum did, you mentioned Ketchum, got his exchange coefficients idea, you know, related to the tidal parameters. We're getting some spatial detail into the design next cut.

And then we're getting into, you see, that tidal flat business that you showed on the Potomac, and we think the same thing is going on right here, in the channels of the Delta, by contrast to the shallower part. What we're doing is exchanging here.

But having done this we find ourselves as I look through the literature, you know, the classic bloom patterns that we have, the spring fall bloom by contrast to a summer bloom. You can get this out of this sort of thing by playing around with the coefficients. You get the pattern.

RATTRAY: What do you put in for an input, P_o?

O'CONNOR: We had to go upstream. There were about nine stations available.

RATTRAY: Did you put in empirical data as you go through?

O'CONNOR: Yes, we took for instance,... they had about nine stations located throughout here in which they had this annual data, so, say one station is here, we took the vicinity of Antioch and used Big Break perhaps as input, that sort of a thing.

RATTRAY: I just wondered how much of these blooms and so forth are reflected by the transport downstream of previous blooms, and how much is really a verification of the model.

O'CONNOR: In one case a fair share. As you can see, you can piece out each one of these things very readily on the computer, so we know what percent is certainly a reflection of upstream. And in one case it was rather significant, that is, like 20 or 30 or 40 percent. And in other cases, less than 10.

DOBBINS: Now is this something that can be modeled in a laboratory? I have a feeling that a lot of these quality problems can be studied in glassware.

O'CONNOR: We got these, Bill, from just that, from the laboratory studies of this type of thing.

THOMANN: I think the problem there is keeping the zooplankton happy for a relatively long period of time.

VLASTELICIA: You asked the question should you go this far in this state of the art report. And you said something and went past it quickly, and I think it's the key to some of the things that ought to be included. And that is that's why our historic data is so important to us. And the data we collect tomorrow is going to be the historic data that we will need. And from Don's point of view in R&D and from ours in the region, I think we need to know the kind of problems that we are going to be faced with, what we should be doing now.

O'CONNOR: I'm with you on that. And that's why I subscribe one hundred percent to what Norb said this morning.

* * *

ESPEY: Dr. Harder.

HARDER: It's getting a little late in the afternoon, and I'm feeling perhaps you ought to treat me to what some after dinner speakers put up with. You give them a large ice cube that they're to hold, and when the ice cube melts, if they haven't put it down by then they're supposed to quit. (Laughter)

This noon we were discussing over lunch with Joe Collins about what is a model anyway. I think it's a good question because we talked about models, and I was a little stumped for a moment to know how to answer that question. Because really we aren't able to model every aspect of what we see in an estuary, but we'd like to model the parts that are interesting and useful to us. Now models have a history that antedate digital computers and analog computers. We've used hydraulic models for many years, and in a hydraulic model we establish a framework essentially of channels which we build as best we can, but that is not the end of the hydraulic model. We don't, in an estuarial model at least, ever have enough information to put the friction in, so we put that in and change it, we put little copper strips in the bottom of the concrete and bend them up and step on them, until the friction is adjusted right, by which we mean that the wave attenuation as you go up the estuary is duplicated in the model from measurements we make in the prototype. Then we think that we've got the model verified, at least so far as tidal velocities are concerned. We've had a lot of experience with hydraulic models, and that gives us confidence that we can use them for other things, I think perhaps a misplaced confidence, but I shouldn't wish really to denigrate hydraulic models entirely. They have a vast and important function in our scientific and engineering life, which I will now relate to you. First, the important thing is that they cost a lot of money. That's very important becuase, if you spend a half a million dollars building a hydraulic model, it justifies going out and getting an enormous amount of field data on sediment transportation, diffusion, tidal ranges, and everything, and then you can spend a half a million dollars going out to get the field data. Once you get the field data, you don't need the model anymore, but then it's served its function of giving the stimulus to getting the stuff you needed in the first place. I think very often we have all the data we need, if we'd only spend the money to get it in our prototypes, and I think that John Vlastilicia has given us a demonstration of that on Bellingham Bay, in which just the measurements of the pollution gave them these nice contour maps.

Now somethimes we don't have a framework on which to hang these coefficients, or these friction coefficients, and if we don't have a framework, we don't know what's going on, we don't know what these equations are, it seems to me it's a vast mistake to assume that. And it's just from that departure and partly along the lines that Don O'Connor was just describing, that we have been working on what I call nonlinear system identification, and there are some copies of a paper in that red box, and some of the consultants along this table have it. It's essentially a black box approach that has just the inputs and outputs, that can be functions of time. But it has what seems to be a small, but it entails an enormous complexity really, in going from linear to nonlinear processes. We have succeeded in identifying nonlinear systems just on the basis of the input and the output, and in one instance we extended it to have two inputs, which were not only nonlinear interactive among themselves, but had a nonlinear interaction between them. This requires a lot of computer time, but not perhaps as much as some of those that we've heard about this afternoon, and it produces results that are remarkably good. In Suisun Bay where we studied salinity, we had a ten year cycle of salinity from which we determined our predictor, and after we reconstructed the salinity over that ten year period, found that we were able to reconstruct it with the root mean square error -- and here the statisticians can see how good things are with a numerical value --

of sixteen percent, that's the rms error between the original and the reconstructed predicted value of salinity. And the salinity ranged from about two or three thousand parts per million to about fifteen thousand parts per million. The independent variable was the flow. Salinity was measured at a point about here, that was the dependent variable, the independent variable was the assumed Delta outflow. Now we knew that there were errors made in the field measurements, so we constructed a completely deterministic parallel to this system, a nonlinear differential equation that we then integrated numerically, which would parallel the behavior of this system, and then we subjected the output of that artificial system to our same analysis and achieved an error of only 8%. That indicates, you see, that the original, which was a combination of what we call analysis errors of the black box analysis and of measurements combined made sixteen, whereas the analysis itself was only eight, suggesting that if these were independent, that the errors of measurement were on the order of 14%. That's the basis of my claim that our method of analysis is more precise in what it contributes to the overall result, has a greater degree of precision than measurements in the field themselves.

Then we applied the same technique to the water levels in the rice growing regions of South Viet Nam, delta of the Mekong River. There we employed two inputs, the river flow into the delta, and the local rainfall, which constituted a different kind of hydrologic input. Within about four or five percent we were able to determine the rms excursion of the water level which is very important to certain kinds of rice cultivation.

This is an entirely different approach, and it is one that does not assume that you have a framework. Now, you don't get something for nothing. If you don't have an equation, this will produce essentially the equations for you, although they're implicit, but you must have more data. If you don't have the framework, and you only have the data, then more data is required to both produce the parameters, the coefficients, and to produce the relationship. But I invite your attention to this. It's something that has really never been published. I just got these together ahead of this meeting and brought a few with me.

I would then second the thought that several people have made already, that if we look just at the techniques of solving some of these equations, we're going to miss the boat rather badly. The real problem before us is to define the kinds of problems that the pollution of estuaries present to us, and I'm very glad that this morning we had the experience of hearing from all of the regions, and hearing from them the kinds of problems they have, many of which cannot be solved by the kinds of techniques that we have, that we're prepared to present, and which are contained in this outline. So I hope that in some fashion, we can extend this report that we are trying to make to include not only the techniques that we presently have, but to indicate in some fashion what the state of the art might be in a couple of years, in which directions the research for solving these problems should go. I very much appreciate Professor Dobbins' comments about being explicit about your assumptions and definitions. I have students who use digital computer programs without knowing what goes on inside. It's another kind of a black box to them, they put something in, they get something out, garbage in, garbage out, sometimes, and I've advocated that every digital computer program ought to have internal to it in some way that you can't pull a switch and turn it off, it ought to have a mandatory printout of all of the assumptions and definitions that are within that program people ought to have. I don't know how you could fix it up so somebody couldn't put in a variable and turn off that printout, but I hope you would be able to do that.

I have a few copies of a report that's a little old, but it describes analog modeling in this delta system, and I have some copies there which I think are more numerous than the others, and you're welcome to. They describe our construction of an analog model for this delta system, now it belongs to the state Department of Water Resources. A figure that is not in that report is how much it cost them, which was $65,000. It is a model in the sense that it solves explicitly the equations of non-steady flow throughout the system. It was designed specifically to determine the net flows in these various channels, that is to say, what kinds of net flows could be expected in the system when various amounts are introduced through wasteways or outlets in the peripheral canal. We found that it duplicates very well certain tidal pumping phenomena around islands. It contains nonlinear friction, the friction depends not only on the square of velocity, but introduces also the hydraulic radius, which is a function of the instantaneous depth. I think that's all I have to say just at the moment.

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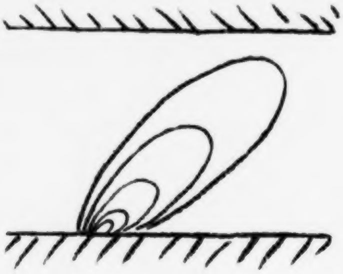
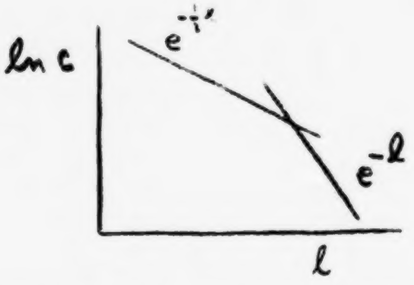
ESPEY: Dr. Pritchard, would you have a few comments?

PRITCHARD: First a few general comments. I think this has been said several times in different ways here among the people that have commented. I would want to suggest first that perhaps the requirements for the development of conceptual models adequate to treat the problems requiring answers is more important now than how one solves the mathematical formulation of the conceptual models after they're obtained. No degree of sophistication in the use of the digital or analog computers or perhaps analog simulators, this I'm less sure of, will correct deficiencies in the conceptual model. We should also consider the use of conventional methods of solution to problems that we face, as well as the more modern use of computers, analog or digital, that is, analytical solutions, of simplified conceptual models, such as Rattray's solution of essentiall a two-dimensional vertical-longitudinal estuary, the similarity solutions, the use of desk calculators and slide rules, this kind of of approach has been left out of the outline. I might comment a little more on that in a specific example in a minute. And further I suggest that the conceptual model to be employed fit the problem requiring solution.

And here I might use an example which will deal with the last two points. We've been concerned here in Maryland with the case of a large nuclear power plant to be located on the shores of the Chesapeake Bay in about the middle stretches of the bay. And we're concerned with, first of all, the local distribution of excess temperature and of other possible pollutants in the plume formed by the condenser cooling water discharge, and the large scale concentration distribution of heat, temperature, and other pollutants, over the major part of the cross section and up and down the bay from the location of the plant site. The first of these, that is, the local distribution, depends critically on the character of the discharge structure. That is, the dimensions of the discharge canal or pipe, the velocity of discharge, also whether this is a surface discharge from the shore or from a canal confined by jetties, or whether it's from a submerged pipeline. The theory of dilution by momentum entrainment in what we call a vent jet gives a concentration distribution after dilution of between 3 to 1 and 10 to 1. And thereafter the natural diffusion processes become important. And when one is dealing with volumes with size of the source term, these are large volumes of flow in this case, over 5000 cubic feet per second taken out of the estuary and put back in, the natural processes of diffusion take a very long time to effectively perform any dilution. There is a common, I think, misconception that the way to deal with a heated discharge is to have it be a very gentle discharge maintained at the surface with as little mixing as possible in order to take advantage of the maximum temperature elevations for cooling to the atmosphere. If you take such a large source of sixteen or seventeen hundred megawatts involving a release of perhaps 12×10^9 BTUs per hour into the environment, and you start considering the environment as simply a cooling pond with no dilution, you find that the areas covered by temperatures, by the higher range of excess temperatures, gets just too large. If you compute the size of the cooling pond for such a system, it is many thousands of acres. In order to use the environment as a receiver of waste heat, at least the approach that I feel is really desirable is to get the maximum temperatures down very rapidly. The way you do this is to promote the mixing and you can't depend on the natural mixing to do it. You've got to design diffusers, and in fact you find when you work on sewage outfalls, say, into the ocean or into an embayment, well, primarily into a large body of water such as a large estuary like the bay, or into the ocean, that you can beat the problem of natural diffusion just by proper design of the outfall. And in the same way you can treat the discharge of heated effluent, but in this case the diffuser gets so massive that one can resort to another alternative, and that is to make use of the velocity of discharge, have the momentum in the discharge to entrain water. The dilution can be obtained quite rapidly.

So that here the theory is one which can be treated with a slide rule and sitting at the desk. Primarily if one has a set of parameterized computer printouts that treat various typical values of the terms, and then just taking the dimensions, speed of discharge, the speed alongshore in the waterway of the water flow which enters the problem, can compute the dilutions fairly well. At a certain point the one phases into the dilution, the natural processes. We know, for example, that, well, if we have such a system, say, let's depict, here's the bay, and here's an input, which say, is a high velocity jet. And we get a certain pattern of distribution, say, of temperature.





So we know that in the jet region, if we plot the log of concentration, or the log of dilution, or the log of inverse dilution, or the log of temperature excess against distance along the plume, that in the jet region, we're going down about distance to the -½ power, and then somewhere we intercept the natural processes where the decrease is ℓ^{-1}. Now the decrease due to natural processes has a higher slope, but it's the slope associated with an infinitesimal source, and we have a finite source, and this doesn't begin to look like it has an infinitesimal source until some distance from the point. And that's why, if we actually went back along this line, we would find that if we depend upon the natural process, we would have much higher concentrations past that intercept.

And the second part of the problem is to be concerned with what is the general distribution in this tidal segment or the cross section by the plant, and how it grades up and down the system. And we don't need a very complex model to tell us this. We have the salinity distributions, which can tell us what the dilution water is here. It comes both from the rivers upstream and the ocean downstream. And in this particular case, the yearly average is something like 50,000 cubic feet per second entering the bay above this point and this is about half sea water, and so we get about 100,000 cubic feet per second of new water here. And it's there, almost rather invariant because when the river flow is high, the salinity goes down, and there's more from the ocean river source and less from the ocean. And if we look at this over a long period of time, it's fairly independent of the volume. The amount of water available to dilute an input distributed across a section is fairly constant over the year, and from year to year, remarkably so. There's another feature of a system like this which is also remarkably constant, and that is, which is a more or less typical two layer estuary, where the plot of the non-tidal velocity with depth looks something like this, this being down towards the ocean in the lower layer, and up the estuary in the upper layer, and the volume of water that is moving seaward in the upper layer through a section is remarkably independent of river flow once we get down into the estuary. Again because of vertical stratification, it varies depending upon the river source. For example in connection with a similar problem on the James River, over a range from about two thousand cubic feet per second to about 12,000 cubic feet per second of flow, the amount of water passing seaward in the upper layer would be the diluting water for a component which because of its density is primarily maintained in the upper layer, such as heat, if you didn't have any salt problem. That is if you drew out of the bottom and discharged to the surface then this process would not necessarily hold. If the temperature was the primary controlling factor on the density of the discharge, then, in the particular case I'm pointing out in the James, over that range of flow rates, which was six full range of river inputs, the amount of flow in the upper layer varied from about 25,000 to about 26,000 cubic feet per second. Only a very small variation, it was actually larger for the lower river flow than the higher river flow. One finds that in this case, they're discharging about 5,000 cubic feet per second, that since there is about 100,000 cubic feet a second going by, the mean concentration would be about 5% of the concentration in the discharge. That is, they are using about 5% of the new water, so that if they heated it 10^{o} the mean sectional temperature due to mixing alone would be a half a degree. The other thing one finds is that if you go into a complex computerized system, or even a non-computer system, to worry about cooling, that if you released this material at say a jet of about ten feet per second velocity, you would have a very small area of the cross section whose temperature is higher than about one degree above ambient. And the cooling of that, the loss of heat from that small part of the plume where you get major dilution, is very small. You are only losing a few percent of the total heat production. Most of your heat loss is at temperatures less than one degree in this kind of situation.

Well, I am just indicating here that one can deal with these problems in a less than highly complex way. You can make decisions in terms of what kind of discharge intake structure, what percentage of new water that can be allowed to be used,...

JAWORSKI: What is the value of the physical model? Isn't there a physical model of this?

PRITCHARD: That is right, but I am not consulting with the company, you see. I serve the state here in seeing that the company's results make sense. They just believe models. Now in this case, the hydraulic model has very great advantage to this kind of situation because people can look at it and they are paying for a demonstration in a way. I can say that there are certain other things, details of the intake discharge structure as far as recirculation for instance, in the models such as they have, can be looked at in more detail in the hydraulic model, and they use it primarily to convince their board of the company that it is worth spending money to have a 540 foot long wide intake structure with a curtain wall to draw water from below 20 or 30 feet to minimize recirculation, to take in water of low biological content, etc. So that you are minimizing the number of organisms killed in passing through the plant, and things like this. So that the companies feel that they need this kind of physical evidence. These structures are so much more costly than the model. The intake design here meant an addition of maybe three million dollars to what they would conventionally do. And they were convinced of this by looking at the model. They wouldn't be convinced by some state representative on a joint committee telling them that it should be done.

While we are on that general subject, you know it is an interesting thing, I just noticed with respect to the last speaker, there seems to be a tendency frequently in this situation of looking at how we look at nature for people to polarize into analytical model groups, and maybe sub-polarizing into use of digital computers, use of analog computers, use of analog simulators, or into groups that favor physical or what I call hydraulic models. And it depends on the group I'm in what people think, how people view, my polarization, because I feel that I'm in the middle, I use all of them. I have certainly used computers probably as much as anybody, with perhaps a few exceptions here that make their life work out of it, but certainly for these problems we use computers all the time. We also use physical models where the models exist and are certainly very convenient to utilize, and because they exist and have been verified over a very wide range of conditions allow their use. I might point out that when we start talking about cost we've got to consider there's a lot of hidden costs in the analytical approach. I remember the first time I ever really tried a large problem with the computer. This was an attempt to do a two-dimensional model of New York harbor and the Hudson River, the flow regime and diffusion, it was a case of trying to predict the dispersion of radioactive material in case the Savannah should have a catastrophe in the New York harbor. At that time, computers weren't very big, this was about five or seven years ago, and I found that a three dimensional model was just out and a two-dimensional model had to end up straightening the river because of memory size. I think this has largely been overcome. But it took about three years, no one had ever programmed that particular type of equation or solution. There was a lot of cost involved in the development. Once that's over then I think the costs become less. The other point I think I would make is that in considering costs, one should consider the output, the results, from the standpoint of savings. I am reminded, the Delaware model has been in existance now certainly fifteen years, maybe it is longer than that. I first used it in about 1953, I think, and it was not brand new then. Over that time, one can sit back and say, we could have said that the Philadelphia district ought to not dispose of their spoil back into the water. The engineers were not convinced of this until they saw it in the model, because they saw that when they put this fine spoil over on the ground, it came back into the channel. The fact that there was about six million cubic yards of new material coming in the system and they were dredging up to thirteen million cubic yards a year to maintain the channel, and the curve every year was just going up, up, up, and they hadn't maintained the channel, still didn't lead them to realize that they were redredging the same material over and over again, plus the new material, which in an estuarine system like that always ends up in the deepest hole, which is this long hole they'd built for the ships to go along. The result is after the demonstration by the model the disposal techniques resulted, so that now they dredge between six, eight, nine cubic yards and over, and maintain the channel's overdepth. This is a very large saving, like millions of dollars a year. Now one could say, well we could have said that reasoning with an analog or any kind of computerized model. It didn't, in fact, take, unless it was demonstrated. And then there are a few problems which I am reminded of, I saw this out in the Columbia. We talked about the Columbia River, and the South Jetty comes out, I've forgotten the shape of the river around here. But the fishing interests had been complaining that the South Jetty should be rebuilt and sunk. It's a $20 million job to rebuild this South Jetty.

Also the question was whether the amount of dredging that had to be done in the entrance in here would be improved if the South Jetty were rebuilt. Now I really don't know, and I don't think anybody can put this question in mathematical terms, whether or not the South Jetty needs to be rebuilt, or whether it would do any good if it were rebuilt, from the standpoint particularly of the cost of maintaining the channel. A hydraulic model was built at a scale of 1 to 500, larger than most of the estuarine models, and the model was verified from the standpoint that the existing shoaling pattern was reproduced, and then they rebuilt the jetty, and it didn't do any good. And so, if that model hadn't been built, and that hadn't been demonstrated, the pressures were, they were going to spend $20 million to rebuild the South Jetty. So that that half million dollars, saved a fair amount of money. I really don't think it could be modeled in any other way.

I think that I did have some comments on this matter of averaging and how this enters into the diffusion coefficients, but I think I'll just pass that up, and get to that another time.

* * *

ESPEY: Thank you, Dr. Pritchard.

I'd like to turn now to the outline that has been distributed in the hopes that we might cover this in some detail before Dr. Baumgartner has to leave, and Dr. Rattray too.

BAUMGARTNER: Yeah, thank you.

ESPEY: I'd more or less like to throw the outline up for discussion. It is merely a tentative outline that our staff and FWPCA has put together. Already some comments have been made. I guess the chapters are techniques, is exactly what they are, and many of the authors to some extent, I guess, have evolved as specific analog or digital technicians. And to some extent, it appeared like a logical format to provide a reasonably well-defined area of responsibility for individuals. Maybe the other approach would be to have water quality as a main section with the techniques within that section. So I'd just like to throw it open for discussion.

I've stated earlier who the authors were. The Introduction is Dr. Pritchard. It is also intended in the Introduction that Dr. Paulik, who has not been able to make our session today, would probably participate also. The analog, Chapter II, Dr. Harder. Chapter III A, Dr. Harleman. III B is Thomann and O'Connor. The Physical Model chapter is Harleman, and the Case Studies is my company, TRACOR. The alternative views and research needs would just be more or less a compilation from the second meeting, putting these into chapter form by our staff based on what you transmit at the meeting.

While I've got the floor here, I'd like to say that it would be our desire and our goal to try to have a rough draft by the first of September. These chapters will be distributed to the participants and another conference held somewhere in the latter part of September. I think that's still a pretty tight schedule, but we'd like to aim for that.

WARD: I think if there's one point that has been made by almost everyone here, it could be that the division of this report into a technique-by-technique approach is artificial, and in many cases invites duplication of some of the topics, and in other cases excludes a thorough discussion of some of these topics. I'd like to suggest an alternative outline. I'll just throw it out in the middle of the floor and invite anyone to jump on it that feels like it. I think the Introduction should be virtually the same in either case. There should be no changes there. I think it would be wise to introduce a second chapter dealing with hydrodynamic models, and I would suggest Dr. Harleman as the author for this one. A third chapter, dealing with water quality models would be virtually the information Dr. Thomann and O'Connor would put together. I would suggest them as authors for that, possibly the section on temperature modeling if we decide to leave that section in.

ESPEY: This would encompass all the techniques?

WARD: Yes, as the authors feel obliged to mention the application of a particular technique, for example this quasi-two-dimensional technique Dick talked about this morning, they could feel free to do so within their own chapter. I think, with regard especially to Bob Thomann's remarks, this report is addressed to people that will be applying these models in the field, and it's appropriate that we have a chapter, I think, on techniques of solution, following maybe the outline that we had anticipated for the entire report. I would suggest Dr. Harder to author this chapter. There's kind of a maverick, this section on physical models, hydraulic models. I think it still needs to be represented in the report -- I don't think anyone would argue with that -- and it doesn't seem to fit in very well with the outline that I just gave, so I think possibly one more chapter dealing specifically with hydraulic physical models as we had in the original outline, once again to be authored by Harleman. The remainder of the report would be virtually the same, a chapter on case studies, a chapter on alternative viewpoints, discussion, and finally the chapter on recommendations.

ESPEY: You don't think physical models is a technique?

CALLAWAY: What do you mean by hydrodynamic models then? Tear the covers off of Lamb and stick it in there? That's kind of a big order.

PRITCHARD: What you're getting at is that, in the present case, you deal with electrical analog in one case, digital computer in the other, you're still going to have to start out writing the basic equations of conservation of momentum, conservation of mass.

WARD: Right.

ESPEY: Wasn't it intended here, George, in the Introduction, that hopefully the basic fundamentals would be set forth?

PRITCHARD: Well, that's what I would hope to do, but in reading Harleman's outline, he is going to repeat that, and I would think that the tendency would be for it to be repeated in each case to some degree.

ESPEY: I agree with you.

PRITCHARD: And in some cases, it's probably got to be to a certain degree, because people are used to thinking and writing in their own framework, terminology. It's going to be hard to get uniformity from that standpoint.

WARD: I think, also, everybody that deals with these things is generally familiar with one form or another of the equations of motion, mass conservation, so we could expect these areas of overlap to be relatively succinct, relatively terse, compared to the remainder of each chapter. So I don't think that would be a serious limitation. Then, too, we could exercise our editorial duties.

(Laughter)

HARDER: George, I think maybe just the group of people you've asked to write these chapters ought to get together without taking other people's time and thrash some of these out for an hour or two, before or after drinks, whichever would be the more successful.

(Laughter)

GALLAGHER: May I ask a question?

O'CONNOR: Yes, please, I mean I think this is most important.

GALLAGHER: Is this supposed to be a historical type of report, just what has been done, what the applications are to estuarine modeling, or projections as to what is possible; or will there be other ideas on simulation, types of things we talked about this morning, applications of linear programming? Do you intend to include this, in other words, possibilities you might say, specific applications?

O'CONNOR: I think that specific applications bit is going to be a difficult one. In order to get something out of that you have to go into quite a bit of detail like the report itself.

BAUMGARTNER: The original intention was not to include any coverage of linear programming or optimization techniques.

GALLAGHER: It's only a historical type survey, I mean what has been done, what are the applications?

BAUMGARTNER: No, several times I've wanted to say something here recently. People have said something about who the audience is, or who this report is being written for. It's not intended to be written for the guys in the regional office to use as some kind of guidelines on how to operate. This is primarily a report to analyze how well we are attacking the problems which have been expressed by the people from the regional offices, guys who are doing most of the operations work, and how well we're stacked up to attack these problems in the future. We see deficiencies, how do we go about solving these deficiencies? Should we put the majority of our effort into mathematical modeling? Should we put more of it into hydraulic modeling? With how much effort should we support analog solutions? We've been stymied with a little bit of popular problems here, too. People who perhaps aren't as familiar with these techniques as many of the people here have a feeling that there are three types of models, and this is how this terminology came about. What kind of efforts should be put into hydraulic models, mathematical models, analog models? Well, that's why we changed this concept a little bit to say various solutions for mathematical models: analog, digital, digital-hybrid, and so forth. So, first of all, I think we ought to appreciate what we're trying to do is make some assessment as to where we ought to put our research effort. To do this we have to have some idea of what the problems are we're trying to solve and what kind of capabilities we have.

GALLAGHER: I was just going to say that the way the discussion has gone so far it seems like we're approaching this from a historical context, of what has been done in the past, and I'm not sure that...

BAUMGARTNER: Well, we've only done this to point the various members here to what we are capable of doing, what we know how to do, what tools we have in the tool box now.

HARDER: With all due deference to TRACOR's modesty in putting themselves here last in the outline, essentially discussing case histories, and also that I think that there's a big improvement that George Ward has made in suggesting an alternative, take one step even further and that is to find out or to establish five or six typical problems that are facing the various regions, even perhaps problems we anticipate, and take each of these five or six problems and establish among the consultants that you have, the best approach to that kind of problem, without regard to whether it's digital computer, analytic, or any other method. Now that's going to take more work, but I think that it would be a far more valuable result.

BAUMGARTNER: I don't know that the problems are that different. I think we're all concerned with water quality problems.

HARDER: But there are some problems that are biological, and some that are advective, and so on. These are the kinds of distinctions...

BAUMGARTNER: Most of the problems that we're concerned with are ones where the substance is not conservative, whether it's chemical, biological, or a combination of both.

HARDER: But then we could distinguish that from the conservative.

THOMANN: Can I ask a specific question, Don? Would you expect, say we had the report in front of us, would you expect that that report would include necessary material so that a man in a regional office could take a first cut at a distribution of BOD or total phosphorus in an estuary?

BAUMGARTNER: I don't think he would get the operational techniques out of this report.

THOMANN: O.k., so you don't see it in a sense as communicating a set of techniques so that somebody could pick it up and use it. I mean if a man wants a solution that is there, that is fairly well described, then the man can pick it up, slip a slide rule...

COMMENT: You will discuss the concepts, though.

BAUMGARTNER: Yeah, that's right, but I don't expect this to be a how-to...

PRITCHARD: Not an engineering handbook.

THOMANN: So you see it as a review of what has been done, and where we are going in the future.

BAUMGARTNER: Yes, right. I think we have to give some serious thought to what kind of time and space scales we have used to solve problems of temperature, BOD, DO, whatever you think are the important problems, coliforms, whether we need to have more sophisticated two-dimensional or three-dimensional models, whether we need to develop more in the way of non-steady solutions, whether we put more effort into the interaction terms, like we've been talking about.

THOMANN: But the answer to that question we have already, don't we? You know, it depends. I think that this Dr. Pritchard's point. There are just some problems that we just don't need two-dimensional time-variable fine grid models.

BAUMGARTNER: Right. I think one of the things we have to do is put this down, and have it available for people in our regional offices to use, and various consultants who want to pursue research in this area will come to us for support, and have some idea of where we think we are and where we need to go.

WASTLER: If I could interject, I'm afraid that since you [to Thomann] have left our little group that it has reached a stage that you have to justify not using mathematical models.

(Laughter)

From time after time after time now, the inquiries we get, the thrust of them seems to be towards, well, can we make a mathematical model of this, not can we solve the problem but can we make a mathematical model.

HARDER: Well, that comment returns us really to what Don had said. If indeed the problems that we are facing are fairly uniform from one place to another, then the original concept of looking at all the different techniques to solve one particular kind of problem would seem appropriate. I had gotten somewhat of a different impression from hearing the regional representatives, and it seemed to me that they had many different kinds of problems, each one of which might call forth one or two, at the most, different approaches.

BAUMGARTNER: Well, maybe I don't see the great difference in the types of problems.

HARDER: Well, for instance, those that have to do with the chemical interaction that were described by the people on the estuary of the Potomac, the different profiles one gets for phosphate and nitrate, these we don't solve by these techniques.

BAUMGARTNER: But as a type of problem I don't think that is any different from the types of problems we have in San Francisco Bay. They're interested in phosphorus...

HARDER: But, I mean in terms of techniques, they are very different problems; the kind of techniques that you would apply, they're different problems.

RATTRAY: In terms of the simplifying assumptions you might be able to do, the scales you work with, the time and space scales would be different.

JAWORSKI: The data requirements.

RATTRAY: Well that is time and space scales, either in the model or the field.

HARDER: And whether it's some kind of algae that is sensitive to salt or not, that is certainly a problem to which the kind of solution is very sensitive. So my suggestion was that we try to identify half a dozen kinds of problems and then with the expertise that you have gathered together, see what technique would be typically applicable to that, and then give some examples and work the whole thing out.

RATTRAY: Or what research needs to be done to develop the hydrodynamic model or the water quality models, space scales and the number of dimensionality to fit those problems.

GALLAGHER: There is one problem. A hell of a lot of our problems are relatively ephemeral. What is a major problem today with us may not be a major problem tomorrow.

PRITCHARD: That's the other thing.

HARDER: Well it may be ephemeral to you but very important to another region.

GALLAGHER: It could be, but I am not just saying for myself alone. From what you have said, I am not sure you can relate it to any specific problem areas. I am not sure I understand what you mean. Do you mean to go into some real detailed discussion of results?

HARDER: No. I think that to make the thing really problem oriented, we don't have to take a specific example and follow the thing through to a complete solution, but we ought to use that as a guide as to the kinds of techniques we'd develop for this report. We'd look first at the problem and the whole multiplicity of the problems, and then pick out the kinds of techniques that would be best applied to that, and perhaps they are not the techniques that we are now applying.

WARD: Don't you think that this could be done within each one of these separate sections?

HARDER: I'd rather see it problem-oriented, and I think that we could find a half of dozen of the problems that we have heard today, a half a dozen different kinds essentially, problems that cry out for different techniques and perhaps even new techniques.

WARD: Of course the lack of generality of a report structured like that might...

HARDER: I should think it would be very specific.

WARD: Well you have six problems, someone is going to have a seventh, and the report is not going to do them any good.

HARDER: Well, I don't mean to make these problems so terribly, terribly specific so that they wouldn't have any application to any other regions.

WARD: Granted.

HARDER: There is a kind of convective problem that is involved with conservative material like salinity, there is a certain kind of technique, certain kind of approach you can use for that. There are problems with one dimensional flow and two dimensional flow, and there are techniques that you could differentiate there. Problems like Don O'Connor was describing with the algae dynamics. I'd like to get together with him, because I think our nonlinear system approach would fit in very nicely with that. That's a problem but it doesn't fit into any of the techniques we've outlined.

GALLAGHER: A lot of this could be taken up in that case history section that TRACOR is going to do.

HARDER: I said we should start with that. Put it first.

GALLAGHER: Maybe what you should do might be,... the object of the game is to define what the priorities are, to discuss what has been done and what the future priorities are.

CALLAWAY: I think most of the principal authors only have five or ten days writing time.

ESPEY: There is a chapter, Recommendations for Future Research, pertinent to FWPCA's mission. Possibly some of the points you have made might well fall within that chapter. I visualize that case studies as being a rather extensive chapter itself attempting to bring together a number of the major studies in the United States, document these studies, point out the history of each, the techniques that were applied, the results, and the data program. We are most sensitive to the data requirements that we've been faced with in the Galveston Bay, so we can very much appreciate these problems. Don, would you or Bob have any comments about the organization?

THOMANN: I generally accept,... tend toward George's approach more than... You know, I don't know what a digital water quality model is. I was just thinking in the light of George's recapping some of the experiences on the Delaware, the same model was used with the analog, hybrid, and digital equipment. You just have to sit down and rewrite the same thing over and over again, and where it really comes home to roost are in some of your case histories. So I generally compliment George's.

PRITCHARD: Would you repeat your, this first, it's a little confusing, if you have a chapter on hydrodynamic models, you're really talking about starting with the basic equations and showing how they are reduced by time averages and space averages to various forms that have been used. I don't know what's left for the Introduction.

HARDER: I am not sure what's left for Techniques.

PRITCHARD: We are really talking about discarded the conceptual framework, from which you take off, at least from the hydrodynamics standpoint. And then there is also one for the conceptual framework with respect to waterborne properties that may be conservative or nonconservative, in particular to nonconservative waterborne properties, the pollutants, and how you fit these into the hydrodynamic structure, and that is the third chapter, I think, which dealt with this, didn't it? Well, the second chapter dealt with the hydrodynamic framework, really, and the next chapter was to deal with... Maybe you could put them on the board so we could be sure that this is right.

RATTRAY: Sort of like many good things, the Introduction is the last thing you write. See what's necessary.

WARD:
I. Introduction
II. Hydrodynamic Models
III. Water Quality Models
IV. Physical Models
V. Solution Techniques
VI. Case Studies

GALLAGHER: That's a good outline. It seems that under that kind of outline, you can state what has been done, you know, evaluate its effectiveness, its merits, its demerits and then define some future priorities as what needs to be done in the regional areas. If you do it the other way, according to the technique outline, the old outline, then you are going to get that repetition.

COMMENT: Well, you haven't really made the switch completely from techniques yet if you are still going to break out the hydraulic method.

HARDER: I think you might just include these techniques, and wipe me off the slate, although I would be glad to talk about something else.

(Laughter)

WARD: I think the techniques, yes, I think we shouldn't leave them out of this.

HARDER: But they are right implicit in the other chapters.

WARD: Well, to an extent they are, but to an extent they aren't.

COMMENT: What about Chapter IV.

PRITCHARD: That Chapter IV is a technique, but it is difficult, however, to not separate it. It is in fact, I agree with you, an analog simulator, the hydraulic model.

RATTRAY: But you never go through the mathematical equations, you don't ever have to write the equations.

PRITCHARD: That's right.

RATTRAY: You have to be able to just form dimensionless parameters, the basic parameters, without knowing the equations at all.

THOMANN: That is always going to hang out there all by itself.

HARDER: I think that in the Introduction, you could give the equations and then each one of these next three chapters could discuss it as it applies to their particular model, these equations, and you wouldn't need Chapter V at all.

DOBBINS: I think this Chapter II, Hydrodynamic Models, needs definition. You could look upon that simply as the hydrodynamics of the estuary without regard to pollution problems. I think that if you look, for example, at what Harleman wrote in that book on estuaries by Ippen, he approached it that way. He wrote chapters on estuaries. One chapter deals simply with the hydraulics of an estuary without getting to pollution at all, then he did a whole separate chapter dealing with pollution analysis.

THOMANN: That is the way I interpreted what you mean.

WARD: Yes.

GALLAGHER: Are we suggesting the same thing, only in a historical context, what has been done and the way we are going?

DOBBINS: Ultimately, if you ever get into real-time mathematical modeling of pollution, you have to have the real-time hydrodynamics established.

WARD: Is this responsive to the kind of report you are looking for, Don?

BAUMGARTNER: We want to be able to answer the question, what kind of problems can be approached now by an analog solution, what kind of problems can we solve with hydraulic models, and what kind by digital solutions to mathematical models, and how much effort should we be putting into these various techniques in our future efforts to solve the problems we can't handle now.

THOMANN: That's hardware.

BAUMGARTNER: Well, it is down there with number V though. It is included.

THOMANN: Yes, I mean it should be discussed, o.k? Obviously. Someone was talking about taking solution techniques out. We still want to be able to answer those questions.

PENCE: Don, it seems to me if you go pretty much with this chapter setup here with the exception that, if you do it in exactly this manner, your solution techniques, your chapter on solution techniques is going to be chopped up by the very nature of the fact. I think it should be far better to say, Chapter II A, the conceptualization of hydrodynamic models, II B, the solution techniques applied to hydrodynamics, III A, Water Quality Models, III B, Solution Techniques applied, IV, the Hydraulics Physical Model, it sort of stands as everyone said, in a different class altogether.

BAUMGARTNER: Maybe I ought to talk about some of the things we didn't want it to do. Give you a little idea about some of the background. We didn't want one guy to write an analysis of the values of three types of solutions, hydraulics, digital, and analog. We didn't want one guy to do that. This is one reason why we have this kind of representation, so people can choose something they are more familiar with, perhaps more expert in, and write about that. Others around here can offer some criticism or challenge as to how well this technique satisfies their problems, so if you are willing to segment this some more, and do it that way, I can see how that would solve the problem.

PENCE: And there is no reason to stop you if you want, you know, people that have some particular experience in solution techniques, you can split them up into your analog, digital, and what have you, and let somebody write, rather than one chapter, maybe three subchapters in the different chapters.

HUSTON: In other words, the techniques will be under hydrodynamic models.

PENCE: Right.

HUSTON: Where would you put physical models then?

PENCE: I would handle it, you have already got the physical models separated out, since they are kind of a different animal, and I would leave them out.

WARD: This is going to require considerably more interfacing among the various authors then. You don't want the person that writes about applications in the hydrodynamic chapter to suddenly change techniques, to suddenly change terminology or notation. One advantage to segmenting the report in this fashion is that each chapter can be unique, can stand alone, virtually. The author can have considerable liberty in choosing his own terminology and in developing his foundations. I agree that's a better approach, but I think that the way that we are trying to write this thing...

PENCE: I can't see one person... As I look at it the most important single part of this is the sorts of techniques from the standpoint of getting at where we are now, what we can do now, and what we will be doing, and I can't see that one person is well enough versed in all the solution techniques to write that chapter by himself, anyway.

WARD: That's right, but don't you think that an important part is also the limitations of the models?

PENCE: Oh yes, but I mean a large part of this is where do we stand right now, what is the state of the art, where are we?

WARD: It looks like we have a choice of sacrificing one or the other. We are either going to compartmentalize our concepts or we are going to compartmentalize our techniques. It's a poor choice, but which one is better?

PRITCHARD: Yes, it would be a shame for somebody to read the solution techniques without finding out the qualifications and the limitations on the model which would appear in the other chapters, that is, the other chapters are going to take the whole gamut, three-dimensions, time-dependent, and bring these down to handleable equations by reducing the dimensions and averaging over time, and if somebody comes along and just reads the solution techniques and doesn't realise the equations he's starting with are a long ways away from the full description of the estuary, that would be unfortunate. But I can see there's problems whichever way you do it.

GALLAGHER: I think it looks fine. That's a good outline.

WARD: Dr. O'Connor, you've been curiously silent. If you'll permit me the vernacular, how does all this grab you?

O'CONNOR: None of it is satisfactory, but I can't offer any better, so I'll use it. Anything we decide is going to be incomplete. Maybe we just should all go back, try the one on the right hand side, all go back, give it a crack, and see what we come up with. We have an idea what we've done, but where do we go? Give it a whack.

HARDER: Here's another modest proposal for an outline of kinds of problems.

1. Hydraulics of Estuaries
2. Hydraulics + Salinity (Conservative)
3. Hydraulics + BOD (Nonconservative)
4. Estuary Biologic Dynamics

CALLAWAY: If you take the one on the right, who are the principal authors again? You still have a conflict between Dr. Pritchard and Dr. Harleman, Chapter I and II. The original intent was to have Dr. Pritchard and Dr. O'Connor write the Introduction.

WARD: No, just Dr. Pritchard.

O'CONNOR: But I do acknowledge that in the Introduction that some of us who are qualified in the quality aspects should have a hand in that, and I think that was what you were getting at.

CALLAWAY: Right.

PRITCHARD: Item 1 and Item 3 are not areas which I can really deal with adequately. Under Part B of the Introduction.

HUSTON: Well, under this kind of framework, the one on the right, those things kind of fall into a different chapter organization.

WARD: Yes, (1) would probably end up in the applications section, and (3) would end up in the water quality section, so they would be very nicely dropped through a crack, as it were.

HUSTON: I'm not sure that this on the right reformulates the original outline very much, except in changing the roman numerals, and grouping the majority of this into what is now V and expanding a little bit on the remainder. It's a little bit of a shifting around, but not a whole lot, really.

CALLAWAY: Let me say one thing. I think the Bureau of the Budget has indicated that they would like to know what are the advantages of analog versus digital versus hydraulic models. Which one costs more and so on.

PRITCHARD: Tell them that that's the wrong question.

O'CONNOR: Absolutely, I think there's unanimity among us on that.

PRITCHARD: I've served on $\underline{n}$ committees which dealt with that same problem, and it's just not the right question.

HARDER: And that's why I plead again that we make the thing problem oriented so that types of problems can have that question answered for them.

WARD: Isn't the question we're really addressing, is it not what does FWPCA need to map out its future research and development effort? Isn't that fundamentally the question?

CALLAWAY: Yes, we've got so much money to put into this, that and the other thing, and how much should we divert to this group versus the other.

WARD: Well, are you going to partition this into analog, digital, and so on?

CALLAWAY: Yeah, if this is a real way to lick the problem. I don't think it is.

WARD: It seems to me that would be the first recommendation we should get out of this conference, that the money not be siphoned into the development of techniques so much as concepts.

THOMANN: Where do you put the use of digital analog techniques, where we sit down and program a digital machine analog components, you know, that act digitally. Some of us use that.

CALLAWAY: Well, I don't know. That's in between them.

HARDER: You write the equations for the analog system and convert them to a finite difference scheme?

THOMANN: Well, the machine does that internally.

HARDER: I'm glad you have a program that does that.

THOMANN: Well, that's our main assault. That's what we use in our modeling, that's what we use to solve that. One writes down an analog diagram and then everything is done digitally. But I think that if anything in the introduction or the closure of this report, it should somewhere be stated that that, by and large, isn't the right question to ask.

O'CONNOR: I intend to have this part of my write-up, and I'm sure all of us do; I'm guessing from what Don said, and from what Don Harleman put in his outline, he's going to take the same tack, so whether it's stated explicitly or implied, I'm sure it's going to get through. I would suggest that both Pritchard and Harleman contribute to II, each of their works, I think, have each made contributions. I don't know who would finally edit that, perhaps you people. But I think each of them can contribute to that area. And I'm still getting back to that point, why don't we just give it a crack. Pass it around to everybody, and I think when we have something in hand, we can comment on it more intelligently, and also at that point write an introduction. No one can write an introduction until they have some better idea of what each of the four authors are going to say. I don't think any one of us are that much aware of each other's work to write an introduction intelligently, aside from the point to saying that the present structure is no good.

WASTLER: I'm wondering if we might not need a glossary stage in this outline. I have a feeling that the term "model" is interpreted in various ways by various people, and I think Hydrodynamic Concepts maybe, and then the various digital models, analog models, and what have you, would be under solution stage. Introduction, Hydrodynamic Concepts, Water Quality Concepts, where your basic equations would be presented and discussed, and down under the Solution Techniques get into all the hardware and software.

O'CONNOR: Well, I see the right hand side has a reasonable viable alternate for a first cut, but it's going to take a lot of work. I don't think we can pursue intelligently or profitably any further here. Myself, I'm willing to spend as much time as you like on it, but we're no further ahead now than we were an hour ago, or from when we first proposed it.

ESPEY: What are you proposing, Don?

O'CONNOR: That we stick to the right hand side, that Pritchard and Harleman do II, Thomann and myself do III, Harleman do IV, Harder do V, then we pass it around to everyone here, get some comments, come back and write an introduction. And then I think that the other possible advantage to that suggestion might be that it would permit us all to get something out rather quickly, that is, you know, stuff we can write rather easily. Then you people can make some attempt to collate and get a first pass. That's where we're going to have the problems, from there on in.

COLLINS: Don, there were two other chapters that were mentioned in here. Alternative views and one on recommendations on research, and that may be the worst one of all to write.

O'CONNOR: Yes, I agree, but I think probably that can be extracted from those four that each of us would write, are in a position to say what we feel are the future directions to take. I know we have considerably different opinions about it, and I'm sure the other ones do too.

COLLINS: Can you write your chapter on water quality without any real interfacing with whoever writes the Hydrodynamics chapter, I mean other than nomenclature, terminology, possibly.

O'CONNOR: No, but I feel I'm sufficiently aware of Harleman's and Pritchard's work, that I think I could write it intelligently. I think it would require editing at that point.

DOBBINS: Would you start with a basic equations, or would you want to derive them yourself?

O'CONNOR: I would just start out with the equation, the usual diffusion-advective source and sink equation, and take off from there. The assumption would be that the hydraulic regime is given.

CALLAWAY: There's still a conflict between Chapter I and Chapter II.

COLLINS: No, I would be an after the fact. II, III, IV, and V would be written...

CALLAWAY: Chapter I, Dr. Pritchard is going to write.

PRITCHARD: The suggestion we have is that Harleman and I both together write Chapter II, just as they together are writing Chapter III, and that the Introduction be written after everything else is done.

O'CONNOR: We could probably sit down for a half a day or a day ourselves and get the outline of I together.

ESPEY: And Chapter V, Dr. Harder would write on solution techniques, is that right?

WARD: This would address mainly digital and analog techniques.

HARDER: I don't really feel that there's much left for Chapter V if the other four chapters do a good job, and I'm ready to resign.
(Laughter)

PRITCHARD: I wouldn't think that the other four chapters would really get to the point of, they would get to the models and all of the assumptions that go into them, after the point that there you have an equation to solve.

HARDER: Do you want a lecture on numerical methods for solving differential equations? That's in every textbook you can find.

THOMANN: Well, in a way, there are really two different sets of differential equations we're talking about, differential equations in the models, a certain solution technique for water quality models may be somewhat different from those for hydrodynamic models.

WARD: I think also the relative advantages and disadvantages can't be overlooked. That's something that's very important to come out of this report.

HARDER: That depends so much on the problem you've got.

WARD: That's true, but don't you think that a setting like this would allow to elaborate on the different kinds of problems and the relative merits and demerits of the techniques?

CALLAWAY: One suggestion is that all the authors go home and write their own outline, send it in, and then we start from there.

O'CONNOR: The first quick dirty cut. We'll cut up our previous papers and reports and tape them together and write a few transitional sentences.
(Laughter)

Espey: Dick has just suggested that if we use the outline here, the Hydrodynamic Models with Harleman and Pritchard, the Hydraulic Physical Models by Harleman, the Water Quality Models by Thomann and O'Connor, Solution Techniques by

Harder, and Case Studies by our group, that each of these authors submit a chapter outline for his individual chapter, send to us as soon as possible for review.

WARD: Well, I think Dr. O'Connor suggested a rough chapter, more than just an outline.

O'CONNOR: I would think this sort of thing would speed up the process and give all of us a better idea.

WARD: Yes, the outline would not give us that much information. O.k., how fast a response should we try to agree upon? Two weeks, three weeks?

ESPEY: How about getting the outline back in a week or so, just to coordinate the participants a little better so there wouldn't be any misunderstanding based on what we think we decided today.

O'CONNOR: You're suggesting in a few weeks an outline, or did I understand you to say in a few days?

WARD: Let's remember that we can't communicate very well with Dr. Harleman. I don't think he'll be able to respond as quickly.

O'CONNOR: Well, perhaps with the outline that he has submitted, Don could probably work within that framework and add, I think, some rather necessary elements of his own work.

PRITCHARD: I was going to say that he's prepared an outline that's a pretty good one. I don't necessarily use the same terms he does.

O'CONNOR: I know, but I think you could add some... I would certainly suggest that under II C some of your own work would be appropriate.

PRITCHARD: I face a little problem with having another job with FWPCA in the next few weeks.

O'CONNOR: Yeah, are you people going to cover out in Jackson Hole the same material?

PRITCHARD: There is a certain redundancy here.
(Laughter)

O'CONNOR: But you're going from the mouth of the estuary out into the ocean, whereas we stay...

PRITCHARD: Well, that wasn't clear certainly in the presentation to us. Really what they're after there for a group to formulate what is known about the coastal regime, which at the time we talked, included estuaries. Well, what does FWPCA need to know. They need to know what is known, and what research needs to be done, and it sounds like to me certainly a question of modeling isn't involved here, more of a question of what we know about the environment, what we need to know about the environment. What is known, and what further research in the environment needs to be done.

O'CONNOR: Perhaps we had better be aware of that. A report will be forthcoming from that too.

VLASTILICIA: What is that report a part of?

PRITCHARD: It's a report a working group will put together under FWPCA funding under the guidance of a steering committee which is a joint steering committee of the National Academy of Science Committee on Oceanography and the National Academy of Engineering. FWPCA came to both academies and said we would like a report that tells us what we need to know about marine waters, how to make our waste loading decisions with respect to the coastal zone, what is known and hence what's left to be known, and some priorities put on the importance of the needed research.

VLASTELICIA: Well, what are you doing Al?

WASTLER: Well, most of these reports were intended to be of help to us. But they're going to be so late that they're probably not going to be.

VLASTELICIA: Well, that's what I wondered. Was this intended to be included in the National Estuarine Studies Report?

WASTLER: Do you remember back last year we were talking about a national conference? This is it, what's left of it. This is sort of a combined thing between our R&D operations and the estuarine pollution study. We're going to mind it as much as we can, but at the same time, we'll be using it as a funding guideline, at least that's what I understand.

O'CONNOR: There is obviously a great deal of overlap. It would be extremely difficult to talk about modeling, without talking about some of these things and conversely.

WASTLER: There is some overlap in almost all of what we're doing by various contracts, but at the same time, we figure it's much better to have overlap and cut things out, than to have holes and have things missing.

O'CONNOR: Good point, there.

WASTLER: Well, it has been pointed out numerous times today, that no matter which way you cut it, you'll have overlap.

PRITCHARD: The outline of the report from Jackson Hole is quite different from this one in that the word "model" never enters the outline. And it's really concerned more with what you need to know in order to develop the appropriate conceptual models, not how you solve them, specifically. Well, in fact I think I can give you what the chapter outlines are going to be. The first one is source terms, essentially, describing what kinds of waste and amounts are going and might be going into the coastal marine environment. The second chapter deals with the effects of the physical processes upon the fate of an introduced pollutant. The third chapter, the effects of chemical and geo-chemical processes upon the distribution, the fate of introduced materials. The fourth chapter, the biological processes, the effects of biological processes upon the wastes and their distribution. Essentially there are three chapters which deal with the effects of the environment on the waste. And then that gets us to Chapter 5, and then there are three chapters which deal with the effects of waste on the environment, physical, chemical, geo-chemical, biological, and then finally a chapter on, I think there was still left in, a chapter on monitoring, or management decisions, or something like that.

O'CONNOR: So there's a reasonable distinction.

PRITCHARD: There's going to be a lot of biology in it. I mean not modeling of biological processes, but what do we know about biological processes.

WASTLER: The thrust of this particular one here, from our standpoint anyway, is toward the techniques of modeling. Of course, with the conceptual material as background, but the thrust in this is more toward the techniques of solution.

DOBBINS: I think a good question to ask here is this, will this document, is it supposed to have more than you could get than by assembling, let's say, a dozen or a half dozen already published texts?

O'CONNOR: That's a good question.

PRITCHARD: And then there's the good question, maybe we don't have to do it.
(Laughter)

WASTLER: Well, published papers are almost always by one or a small group of authors, their own little slant. What we are really trying to get is a nationwide state-of-the-art message on what the state of the art of modeling is.

DOBBINS: Well, I still think that my point is, is the objective something more than what you have by a collection of published papers? The people who write it probably haven't had this in mind somehow or other. In other words, is it an assembly of what we know and a representation in a different manner? I don't know, I'm just asking a question.

WASTLER: I would hope that it would be a synthesis, rather than an assembly.

HARDER: It seems to me that what you're really looking for is not the knowledge that is contained in all of the papers, but some wisdom in the relative usefulness of the four different methods. And I don't know how you can do that unless you make it problem-oriented, if I can stick my oar in once more.

(Laughter)

ESPEY: Let me attempt to summarize then and possibly adjourn this meeting. There will be an organization followed along these lines, which we would like to suggest. Dr. Harleman and Dr. Pritchard co-author the Hydrodynamic Model section, the Hydraulic Physical model section will be authored by Dr. Harleman, the Water Quality section by Dr. Thomann and O'Connor, the Solution Techniques will be co-authored by Harder and possibly Orlob, and the Case Studies, as before, by my company. We would like to suggest that an outline be submitted to me by July 2, delineating the individual author's chapters, for distribution and coordination with the other participants to minimize duplication and discrepancies. I would suggest this outline be in the detail that Dr. Harleman has already submitted, something of that order of magnitude.

QUESTION: Did you not suggest that the Introduction be written by Pritchard and O'Connor?

PRITCHARD: No, the Introduction is going to be written after the rest.

COMMENT: I think this is going to be by committee to some extent.

ESPEY: I would also like to open up the discussion at least briefly to any feelings on the next meeting location. Does anyone have any suggestions, other than the obvious?

(Laughter)

If there're no suggestions, I suppose we will inform you of the next location.

(Laughter)

O'CONNOR: We aren't going to come to Austin.

(Laughter)

ESPEY: I anticipate that it will probably be on the east coast because there are more members participating from this area.

THOMANN: That's exactly the reason why it should be on the west coast.

O'CONNOR: Why sure.

WASTLER: There are free facilities in Corvallis.

CALLAWAY: On the other hand we'd better see how TRACOR feels about trotting half the people here across the country.

COMMENT: How about Silver Springs or Rockville?

CALLAWAY: Jackson Hole is popular this time of year.

PRITCHARD: Yeah, you might put it at the end of the Jackson Hole meeting. It's hard to get out of there. I can't get reservations out of there, I can get in, but I can't get out.

(Laughter)

ESPEY: Well, we'll work this out with both the regional people and the participants.

PRITCHARD: The nearer here the better for me, since I travel a lot.

WARD: I have one other request to make, especially of the FWPCA people. Our little chapter on Case Studies. Without slighting our abilities, we took that mainly because there was more legwork involved than insight, and if some of the federal people could help us with some of the legwork, we would surely appreciate it. We know of some studies. Some ways to get reports take time, if you have to write to the state offices or things like that. If you have copies of these reports that you are free to send us, we would appreciate receiving them. And here also the experience of the regions is even more important than the reports, I believe.

VLASTELICIA: Are you going to use any studies of your own, interpretative case studies, or are you planning to put together studies that are already completed?

WARD: Considering the constraints of this budget, we can't do much more than put together what's already been done. If someone suggests a problem that we can answer, we'll probably leap in there, but I think that's unlikely.

ESPEY: Are there any further comments?

WARD: Would anyone like to suggest a date for the next meeting?

ESPEY: We were thinking of the latter part of September.

WARD: A lot of schools kick up that last week in September. It might be better to defer it to, say, the first week in October. Is that any problem at Manhattan?

O'CONNOR: No, we start in early September.

WARD: What about Johns Hopkins?

PRITCHARD: We're starting early in September in order to get the first term over by Christmas. We've had a reorganization of the calendar.

WARD: So late September would be as convenient as any other time?

PRITCHARD: At any time after the school year begins.

O'CONNOR: Beware of the number of meetings that are going on at that time. Water Pollution Control Federation is having their national meeting in Dallas. That's the week of October 5 or 6. You want to meet that week?
(Laughter)
We're on the program, and I dare say a number of people here will be at that.

ESPEY: I think that's a very good suggestion.

O'CONNOR: ASCE has their national meeting I think, the week after that though.

ESPEY: Well, we'll contact you regarding that. I guess we'll adjourn the meeting as of now.

* * *

APPENDIX I

ORIGINAL SUGGESTED OUTLINE

I. INTRODUCTION

- A. Objectives and Summary
- B. Fundamental Principles of Water Quality Modeling
    1. Philosophy and application in management
    2. Outline of the physical processes governing the movement, mixing and distribution of a substance (or parameter) in an estuary.
        - a. advection
        - b. dispersion
    3. Parameters employed as index of water quality and their treatment in a model.
- C. Bibliography (not annotated)
- D. Discussion

II. ELECTRICAL ANALOG AND ANALOG-DIGITAL HYBRID METHODS

- A. Introduction
- B. Analog Modeling of Etuarine Processes.
    1. Hydraulics
    2. Water quality
- C. Analog-digital Hybrid Modeling
    1. Hydraulics
    2. Water quality
- D. Advantages and Disadvantages
- E. Examples of Application
- F. Annotated Bibliography
- G. Discussion

III. DIGITAL COMPUTER MODELING

- A. Hydrodynamic Modeling

1. Fundamentals
2. Alternative formulation of equations in one, two, three spatial dimensions, steady-state and time varying.
3. Orders of magnitude of terms for various estuarine

B. Water Quality Modeling

1. BOD, DO, nutrients
   Formulation and treatment of the equations
2. Temperature modeling

C. Numerical Considerations

1. Accuracy
2. Round-off, stability

D. Annotated Bibliography

E. Discussion

IV. PHYSICAL HYDRAULIC MODELS

A. Fundamentals: Scaling, Distortion

B. Application to Pollution Problems

1. Dispersion phenomena
2. Non-conservative tracers: modeling of BOD distribution, reaeration processes, etc.
3. Examples of application

C. Advantages and Disadvantages

D. Annotated Bibliography

E. Discussion

V. CASE STUDIES

A summary review of major applications of models of all kinds to specific estuary problems. Will include in particular the two topics: streamflow regulation and physical modification. An annotated bibliography and discussion section will be appended.

APPENDIX II

OUTLINE OF CHAPTERS III-A AND IV

D. R. F. Harleman

CHAPTER III-A HYDRODYNAMIC MODELS

I. Introduction

This chapter will deal with mathematical models for estuaries in the form of conservation of mass, momentum and continuity equations. The distinction between hydrodynamic modeling and water quality modeling (Ch. III-B) is rather arbitrary. I propose to discuss all three types of equations; however, in the conservation of mass equation I will consider concentration as a basic varaible which, in general, may be a function of time and certain spatial dimensions. The conservation of mass equation contains the unsteady concentration term, the convective mass transport (depending on velocity terms which describe the fluid motion), the diffusive or dispersive mass transport, and it may also include a "first order" decay term without additional difficulty. This is intended to provide the necessary "hydrodynamic" framework for Chapter III-B.

The development will stress the basic concept that hydrodynamic modeling involves the simultaneous representation of mass transport by the fluid motion (advection/convection) and by conduction (diffusion/dispersion). The diffusion/dispersion terms always depend on the way in which the advective/convective motion has been discribed and it is impossible to separate the two mechanisms. The importance of the diffusion/dispersion terms decreases as the accuracy of description of the advective/convective processes increases.

The fluid velocity terms in the conservation of mass equation can be obtained either by direct measurement, by indirect measurements involving use of the continuity equation or (for predictive purposes) by a combined solution of the momentum and continuity equations.

The three equations, conservation of mass, momentum and continuity, will be discussed in terms of the number of spatial dimensions to be considered. Initial and boundary conditions will also be discussed. A clear distinction will be made between various steady, quasi-steady, and unsteady forms of the equations.

In order to discuss specific results, it will be convenient to allow the concentration variable to represent either salinity, dye (or radioactive material) or BOD from a single input source. All other water quality considerations should be treated in Chapter III-B including: Complexities of the BOD distribution (due to multiple or distributed sources, bottom sludge deposits, etc.); temperature distribution; dissolved oxygen distribution and all other chemical and biological effects.

II. Two-dimensional Mathematical Models for Estuaries

A. Unsteady--Instantaneous Tidal Time.

1. Lateral homogeneity
   (tidal motion in horizontal and vertical directions)
   (effect of bottom friction on tidal current)
2. Vertical homogeneity
   (tidal motion in horizontal plane; Coriolis effect)

B. Steady--Averages Over a Tidal Cycle.

1. Lateral homogeneity

C. Applications of the Two-dimensional Models.
(Digital methods)

III. One-dimensional Mathematical Models for Estuaries

A. Unsteady--Instantaneous Tidal Time.

1. Variable-area channels
   a. Basic equations and discussion
      Continuity
      Momentum
      Conservation of Mass

   Discussion of coupled equations and simplifications:

   (i) tidal velocity from continuity and momentum equations for prediction and non-linear effects,

(ii) tidal velocity from continuity equations only,

(iii) tidal velocity from sinusoidal assumption

Magnitude of longitudinal dispersion coefficients.

(i) in salinity intrusion region

(ii) in tidal regions without density gradients

b. Applications--Digital methods

2. Constant Area Channels (coupled equations--digital methods--analytical solutions--numerical evaluation of integral equations.)

B. Quasi-steady State (tidal slack time)

1. Effect of tidal excursion
2. Salinity Intrusion (const. and variable area) Analysis Prediction of changes in salinity intrusion due to variation of flow and geometric parameters.
3. Dye dispersion (const. and variable area)

C. Time Average over tidal cycle

1. Analytical solutions for variable areas and constant area channels.
2. Numerical methods and Applications. (Discussion will include only advection-dispersion and first order decay)

IV. Conclusions

Chapter IV-Physical Hydraulic Models

I. Introduction

Discussion of areas of application of physical models. Need for geometrical distortion in tidal models. Reynolds number effects in models. Necessity for emperical determination of model roughness. Need for field data for model verification and roughness adjustment. Principles of dynamic similarity. Value of physical models in certain complex problems.

II. Tidal Motion

Froude similitude from momentum and continuity equations, roughness conditions, adjustment and verification from tidal stage, phase and velocity observations. Reproduction of Coriolis effects in tidal models.

III. Salinity Intrusion

Saline wedge and mixed estuaries. Induced vertical mixing in tidal models. Adjustment and verification of salinity intrusion. Density differences in model and prototype. Effect of variations in fresh water discharge. Reproduction of salinity and velocity distributions in longitudinal and vertical directions.

IV. Diffusion and Dispersion of Effluents

Conditions for mass transfer similitude. Distortion of longitudinal dispersion effects in tidal models having geometric distortion. Validity of dye dispersion studies, interpretion of model observations.

Other limitations of physical models. Difficulties with mass transfer, chemical and biological processes.

Examples and applications.

V. Conclusions